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PHYSICAL REVIEW.

VARIATION IN ELECTRICAL CONDUCTIVITY OF METALLIC WIRES IN DIFFERENT DIELECTRICS.

By Fernando Sanford.

In a paper published in 1892, entitled "Some Observations upon the Conductivity of a Copper Wire in Various Dielectrics,"[1] I called attention to the fact that, with the copper wire used by me in the experiments referred to, the conductivity varied in different liquid and gaseous dielectrics by an amount approximating in some cases to 0.2 of one per cent of its air resistance at the same temperature.

In a later paper, entitled "A Necessary Modification of Ohm's Law,"[2] I referred to a similar observation made upon the conductivity of a silver wire.

The apparatus used in the experiments referred to in the above-mentioned articles consisted of a copper tube 120 cm. in length and 2.5 cm. in internal diameter, closed at the ends with copper plugs provided with stopcocks. To the inside end of one end plug was connected a copper wire 1 mm. in diameter, which was stretched lengthwise through the center of the tube, and passed out through an insulating plug in the center of the other end plug. The resistance of the wire and tube was measured at various tem-

[1] Leland Stanford Jr. University Publications. Studies in Electricity, No. I.

[2] Phil. Mag., January, 1893, Vol. 35, p. 65.

peratures when the tube contained air, and again when the tube contained other liquid and gaseous dielectrics. A side tube midway between the ends of the long tube served to admit a thermometer, by means of which the temperature of the dielectric inside the tube and near the wire was measured.

Some fifteen different liquid and gaseous dielectrics were used, and, in general, the conductivity of the wire was found to vary from its air conductivity at the same temperature by an amount which was different for each dielectric used, and which was constant for any particular dielectric.

The accuracy of the observation was attacked upon both theoretical and experimental grounds (see especially *The Electrician*, London, of January 13, 1893, and Professor Henry S. Carhart in the PHYSICAL REVIEW, Vol. I., p. 321), and though it had been verified by me with several different wires, and though I had collected considerable material bearing upon the nature of the phenomenon, I have thought best to publish nothing further upon the subject until the observation had been verified by some one outside of my own laboratory.

I have recently received a memoir by Professor Grimaldi and Dr. Platania, of the University of Catania,[1] giving the results of a long and very careful series of experiments upon the resistance of a copper wire in air and petroleum. These researches have been conducted with greater refinement and more delicate apparatus than have any previous experiments upon the same subject with which I am acquainted. The results show a degree of concordance quite remarkable, when the difficulties of the determinations are taken into account. In every case the resistance of the wire in petroleum was found to be less than in air. This difference in resistance was, however, when reckoned as a percentage of the whole resistance of the wire, only about one-twelfth as great as in the case of the wire with which the phenomenon was first dis-

[1] Sulla Resistenza Elettrica dei Metalli nei diversi Dielettrici. Memoria del Dott. Giovan Pietro Grimaldi, Professore di Fisica nella Regia Università di Catania, e Dott. Giovanni Platania, Assistente al Gabinetto di Fisica della stessa Università. Parte I. Ricerche sulla variazione di resistenza del rame nel petrolio. See also, Dal Bollettino mensile dell' Accademia Gioenia di Scienze Naturali in Catania Fascicolo XXXVIII., seduta del mese Dicembre 1894. Also Nuovo Cimento July 1895.

covered by me; but the total variation in resistance amounted, taking the mean of all the determinations, to 0.00002 ohm, while in the same dielectric with the wire originally used by me it amounted to 0.00006 ohm. The wire used by Signors Grimaldi and Platania was 304 mm. in length, and 0.22 mm. in diameter. Its variation in resistance per unit length was accordingly greater than in the wire used by me, while the corresponding surface exposed was less than one-fourth as great.

The intelligence and care shown in the published work of Signors Grimaldi and Platania, and the remarkable uniformity of their results, make it improbable that the existence of the phenomenon will again be questioned.

The method and apparatus used in the following experiments were practically the same which I described in my former papers. The wires whose conductivities were to be measured were stretched lengthwise through the tube used in the earlier experiments, and were sometimes soldered and sometimes connected by means of a binding screw to the inner end of one plug, and were carried through the insulating plug at the other end of the tube. The resistance of the wire and tube was at first measured by means of a Hartmann & Braun resistance box and bridge, with bridge arms of 1 : 1000, the comparison coils being divided to tenths of an ohm. Later, a Nalder Brothers resistance box and bridge, with arms of 1 : 10000 was used, the comparison coils being divided to tenths of an ohm. The galvanometer used gave a noticeable deflection for a change in resistance of 0.00002 ohm, and later for 0.00001 ohm.

The temperature of the interior of the tube was measured by a standard thermometer graduated to 0.1°, inserted into the side tube with its bulb in contact with the wire, and read through a telescope which enabled the observer to estimate to 0.01° with a fair degree of accuracy.

The wire first used had with the tube a resistance of 0.0335 ohm at 18° C., and a temperature variation of 0.000014 ohm for one-tenth of a degree centigrade. By reversing the current it was possible, even with the least sensitive galvanometer used, to estimate the resistance accurately to 2 in the fifth decimal place; and

with the galvanometer used later, an error of 1 in the fifth decimal place would not be made, even without reversing the current. This, while not the highest attainable accuracy in resistance measurement, is sufficiently accurate for the work, since the temperature of the wire could not be known with certainty to a greater accuracy than 0.1°. Had the measurements been made at a temperature differing greatly from the room temperature, or had the wire been perceptibly heated by the current, even this degree of accuracy would be improbable, since what was really observed was the temperature of the thermometer in the tube near the wire, and the small wires used would almost certainly change in temperature more rapidly than the thermometer. To guard against a possible error of this kind, the measurements were all made at the room temperature, which very rarely varied so much as 10° in twenty-four hours. The measurements were also made at all hours of the day, and sometimes late at night, in order that approximately the same number might be made with rising as with falling temperature.

To guard against the error due to heating of the wire by the current, the resistance of the wire at the same apparent temperature was observed for a wide range of current strength, and the current strength was kept far below the point where the resistance of the wire seemed to increase. The zero method of measurement was used throughout, and the current was passed through the wire only long enough to enable the galvanometer deflection to be read. In the first work, in order to secure a constant current strength, a battery of thirty-two silver chloride cells with high internal resistance was used with 100 ohms additional resistance in the battery circuit. Later, one Edison-Lalande cell was used with a resistance of from 50 to 100 ohms in the battery circuit.

That temperature fluctuations did not cause serious error may be seen from the data published with the first mentioned paper. The curve shown in Fig. 3 of that paper is reproduced here as Fig. 1. It represents the resistance of the wire in air, and in the gasoline burning gas used in the laboratory. There are twenty-six measurements made with the tube filled with air. These were made at irregular intervals for six days, the tube having been in the

meantime filled with burning gas, and seventeen measurements made on three successive days with the wire in this dielectric. The tube and wire were neither disconnected nor moved in changing the dielectric. The gas was admitted through one of the end stopcocks, which was attached by a rubber tube to a gas cock in

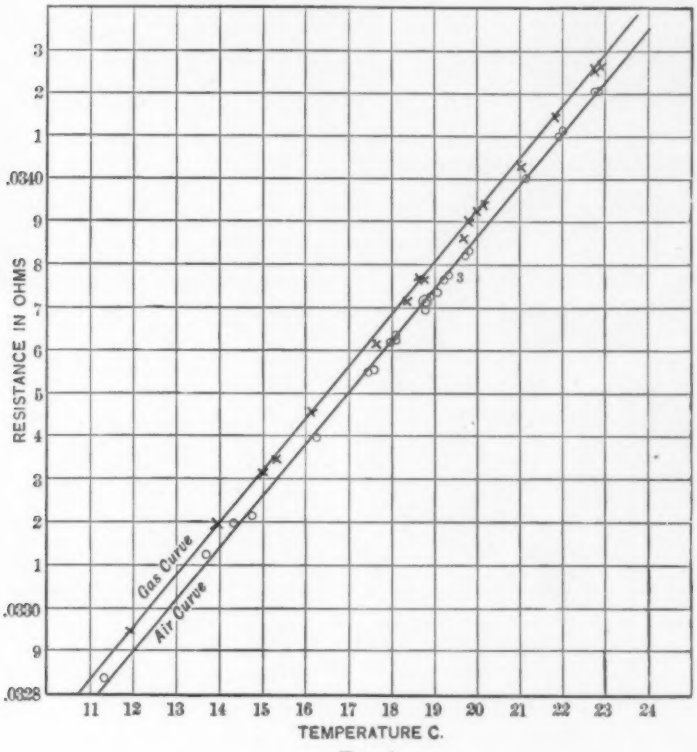


FIG. 1.

the laboratory, and was removed by blowing air through the tube with a bellows.

Of the twenty-six points platted for the air measurements, nineteen were made before the tube was filled with the gas, and seven were made after the gas had been removed. The greatest distance of any single air point from the curve corresponds to a resistance of 0.00002 ohm, or a temperature variation of 0.14°. In the seventeen measurements made with the tube filled with the burning gas, the greatest distance of any point from the curve corresponds to a

difference in resistance of 0.000025 ohm, or a temperature variation of 0.15°, while only one point in the seventeen is more than 0.00001 ohm from the curve.

The average difference in the resistance of the wire and tube with the dielectrics named was 0.000058 ohm, corresponding to a temperature difference of 0.41°, while the nearest approach of any gas point to the air curve represents a difference in resistance of 0.00005 ohm, corresponding to a temperature difference of 0.36°.

That the temperature of the wire can be known with sufficient accuracy when the above precautions are taken is shown by the data published in the article by Professor Carhart, of which mention has already been made. In the investigation reported by Professor Carhart, a tube and wire combination similar to the one above described was heated up to 30° C. by warm water, and a series of resistance measurements was made while the temperature was falling to 20° C. Even with this arrangement, and with a thermometer reading only to half degrees, and a wire whose resistance varied by 0.000018 ohm for one-tenth of a degree, the different measurements show a very uniform decrease of resistance for the estimated decrease of temperature.

I lay special stress upon this point, because in the editorial article in *The Electrician*, previously referred to, the writer charges that it is "a great assumption to suppose that the temperature of the wire is the same as that of the dielectric," and says: "The wire is, of course, being heated by the current, and a slight change of temperature would necessarily result from change of emissivity, owing to the different thermal properties of the various dielectrics used." This explanation would hardly have been offered had the author read with any care the article which he was criticising, since it would make it necessary to assume from the data there given that the temperature of the wire in petroleum was 0.43 of a degree *less* than in air, while in petroleum containing a little wood alcohol its temperature with the same current through it as before was 0.5 of a degree *greater* than in air. As a matter of fact, increasing the current strength fourfold did not alter the apparent resistance of the wire at the same temperature. In the very careful provisions made for maintaining the wire at constant tempera-

ture by Signors Grimaldi and Platania, this possibility of error was still further eliminated, so that it is certain that, whatever else may be the cause of the phenomenon, it is not due to a temperature variation of the wire.

The following table gives the change in resistance observed in the above-mentioned wire in eight different dielectrics, and the temperature change necessary to account for such variation in resistance upon the hypothesis advanced by *The Electrician.*

Dielectric.	Resistance change.	Equivalent temperature change.
Petroleum	−0.00006 ohm	−0.43 degree
CS_2 and turpentine	−0.00003 ohm	−0.2 degree
Wood alcohol and benzine . .	+0.00005 ohm	+0.36 degree
Absolute alcohol	+0.00006 ohm	+0.43 degree
Wood alcohol and petroleum .	+0.000075 ohm	+0.53 degree
Laboratory burning gas . . .	+0.000058 ohm	+0.4 degree
Chloroform vapor	+0.00005 ohm	+0.36 degree
Ether vapor	+0.000083 ohm	+0.6 degree

Following the publication of my first paper upon this subject, a number of measurements were made with a silver wire 1.1 mm. in diameter, using as dielectrics air, petroleum, wood alcohol, absolute alcohol, ether vapor, chloroform vapor, laboratory burning gas, and a mixture of wood alcohol and petroleum. While the same phenomenon was observed as with the copper wire previously used, the variation in resistance was not so great. The most important difference, however, was in the different order of arrangement of the dielectrics with reference to their influence upon the conductivity of the wire. While the copper wire had shown a lower resistance in petroleum than in air, the resistance of the silver wire was very slightly increased in petroleum; and whereas the resistance of the copper wire had been greater in sulphuric ether vapor than in any other dielectric used, the resistance of the silver wire was less in the ether vapor than in any of the other dielectrics. The following table will show the results of the measurements made upon the silver wire:—

DIAMETER OF WIRE, 1.1 MM.; RESISTANCE OF WIRE AND TUBE AT 18° C., 0.02705 OHM; TEMPERATURE VARIATION FOR 0.1 DEGREE, 0.000009 OHM.

Dielectric.	Resistance change.	Equivalent temperature change.
Petroleum	+0.00002 ohm	+0.2 degree
Absolute alcohol	+0.00003 ohm	+0.3 degree
Wood alcohol	+0.000045 ohm	+0.5 degree
Wood alcohol and petroleum .	+0.000046 ohm	+0.5 degree
Ether vapor	−0.000025 ohm	−0.3 degree
Chloroform vapor	+0.000025 ohm	+0.3 degree
Laboratory burning gas . . .	Uncertain. Resistance slightly greater than in air	

The results hitherto mentioned have been referred to in previous publications. Those which follow were made under practically the same conditions, except that the temperature of the room was more constant, and rarely varied so much as five degrees in twenty-four hours. The time used in making a single set of comparison measurements varied from one to two weeks, during which time several measurements were usually made each day. Care was taken, as before, that approximately the same number of measurements should be made with rising as with falling temperatures.

The copper wire already mentioned will hereafter be referred to as C_1. Its diameter, as before stated, was approximately 1 mm. In order to observe any possible effect of an increase of surface of the wire, the same tube was provided with a copper wire, of 1.27 mm. in diameter, hereafter designated as C. The resistance of this wire and tube when the tube was filled with air at 20° C., was 0.02618 ohm. The resistance when the tube was filled with the mixture of air and gasoline vapor used as burning gas in the laboratory was 0.02626 ohm, showing an increase in resistance of 0.00008 ohm.

With the wire C_1, which was of the same length in the tube, the resistances in these two media at the same temperature was respectively 0.03375 ohm, and 0.03381 ohm, showing a change in resistance in the burning gas of 0.00006 ohm. This showed that while the surface of the wire had been increased about 60 per cent,

the difference in resistance had been increased about 30 per cent. As this is the greatest variation observed in any wire with the change in dielectric, I give in Fig. 2 the curves representing the resistance of the wire in the two media. The data upon which the figure is based are given below. The only change made in the apparatus when the gas was introduced was the opening and closing of the stopcocks in the end plugs, as the tube was already attached to the gas cock in the laboratory.

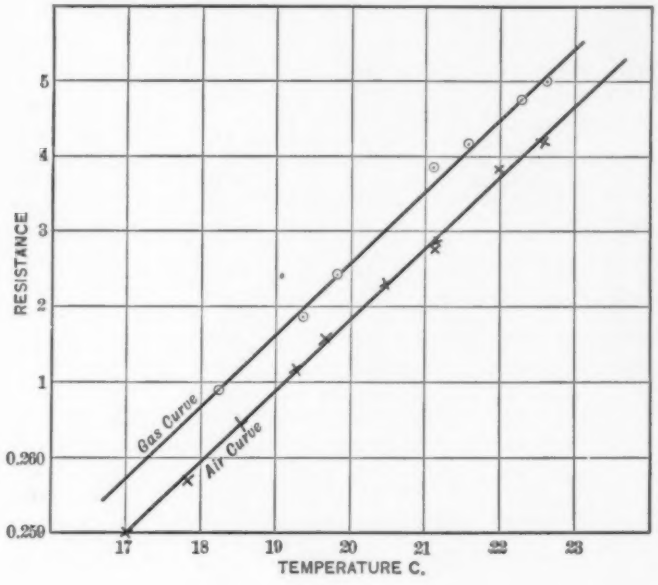


FIG. 2.

In order to determine whether all the dielectric in the tube was concerned in the resistance change, or only a thin layer of it in contact with the wire, the wire was withdrawn from the tube, and its surface was very thinly coated with paraffine oil, after which it was replaced in the tube, and a second set of measurements was made with the same dielectrics as before. These measurements gave exactly the same resistance in the two dielectrics. This, together with the fact, mentioned in my first paper, that after the tube had contained a liquid dielectric the wire frequently required a considerable time before it returned to its air resistance,

has led me to believe that the phenomenon is due to a very thin film of the dielectric adhering to the surface of the wire, and possibly absorbed to some extent by the wire. It was especially noticed that after the tube had contained ether vapor it required a long time, frequently several days, to return to its previous air resistance. In one case, the wire continued to show an augmented

COPPER WIRE C_2, 1.27 MM. IN DIAMETER.

Date.	Hour.	Dielectric.	Temperature.	Resistance.
Nov. 4	12.30	Air	21.1–	0.02629
Nov. 4	3.30	Air	22.6	0.02642
Nov. 4	5.45	Air	22.0	0.02638–
Nov. 5	10.30	Air	18.5	0.02604–
Nov. 5	11.45	Air	19.25	0.02612
Nov. 5	5.10	Air	20.5	0.02623
Nov. 7	10.00	Air	17.0	0.02590
Nov. 7	11.15	Air	17.8	0.02597
Nov. 7	2.00	Air	19.7	0.02616–
Nov. 7	5.00	Air	21.1	0.02628+
Nov. 7	5.10	Burning gas	21.1	0.02639
Nov. 8	8.45	Burning gas	18.2	0.02609
Nov. 8	11.00	Burning gas	19.8–	0.02624
Nov. 8	1.45	Burning gas	21.6	0.02642–
Nov. 8	3.20	Burning gas	22.3	0.02647
Nov. 8	5.00	Burning gas	22.7	0.02650
Nov. 9	9.10	Burning gas	19.3	0.02619+

resistance, apparently due to ether vapor, after air had been drawn through the tube by a filter pump for more than twenty-four hours, and it returned to its air resistance only after the tube had been filled with alcohol for some time and was then emptied and dried. It was also noticed that during all this time the odor of the ether could be detected in the tube.

In order to observe whether any effect would be produced by increasing the surface of a given wire, another wire, C_3, 1.27 mm. in diameter, was cut from the same spool as C_2, and was hammered flat to increase its surface. This wire was placed in the tube, and its resistance was measured in the two dielectrics which

had been used with C_2, but no increased resistance could be observed in the gas. The wire had been much hardened by hammering, but what change had been produced in its surface is not known.

The next wire used was 1.66 mm. in diameter, and is designated C_4. Instead of giving an increased difference of resistance corresponding to its increase of surface, its resistance change in burning gas was less than that of either C_1 or C_2. Its resistance in air at 18° was 0.01915 ohm, and in burning gas at the same temperature it was 0.01917 ohm, showing a difference of only 0.00002 ohm. The same wire was tested in absolute alcohol and in petroleum poured in after the alcohol had been drawn off, in both of which dielectrics it gave a smaller difference of resistance than did C_1. In alcohol this difference amounted to about 0.00004 ohm, and in petroleum with trace of alcohol to about 0.00005 ohm.

Another copper wire, C_5, 1.27 mm. in diameter, was cut from the same spool as C_2, and was amalgamated upon the surface by immersing it a short time in a solution of mercuric chloride. Its resistance at 20° was 0.02640 ohm in air, and 0.02646 ohm in ether vapor. In freeing the tube from ether vapor, one of the contacts was loosened, and the apparatus was disconnected and remained standing unused from Feb. 9 to March 6. It was then reconnected, and measurements were made with air, burning gas, petroleum, and a mixture of wood alcohol and petroleum. By the time these measurements were undertaken the surface of the wire had become blackened, instead of showing the bright mercury surface, and the resistance of the wire had appreciably increased. All of the following measurements made with this wire showed much more than the usual irregularity, but in the case of air and petroleum this irregularity was not great enough to bring any point made with the wire in one dielectric upon the curve made with the wire in the other dielectric. The resistances at 20° were as follows: In air, 0.02660; in petroleum, 0.02656. In the case of burning gas and the mixture of wood alcohol and petroleum, the irregularity was so great that some of the points fell upon one side of the air line, and some upon the other, and the measurements were accordingly rejected.

The apparatus was next fitted with a copper wire, C_6, about 0.35 mm. in diameter, and its resistance was measured in air, burning gas, and petroleum. In this wire, a change of temperature of 0.1° corresponded to a change in resistance of 0.00008 ohm, so that any resistance change in the different media was masked by possible temperature differences. The mean of fifteen measurements in air, and five in burning gas, seemed to indicate an increase in resistance in the gas of about 0.00001 ohm, but this was probably purely accidental. In petroleum, no difference could be observed. Evidently, the detection of the resistance changes in such fine wires requires a more delicate means of measuring temperature than was afforded by my apparatus. That this difference in resistance may still be considerable in much finer wires than any used by me is shown in the work of Signors Grimaldi and Platania, where a wire only 0.22 mm. in diameter gave a difference in resistance per unit length as great as my wire C_1.

Silver Wires.

The measurements made with the silver wire referred to in my paper in the *Philosophical Magazine* have already been mentioned. The actual resistances observed at the same temperature in the various dielectrics used are given below. These numbers are the mean of three series of measurements, but not all the dielectrics were used in each series.

SILVER WIRE, S_1, 1.1 MM. IN DIAMETER.

Dielectric.	Resistance.	Dielectric.	Resistance.
Air	0.02722 ohm	Ether vapor . . .	0.02719– ohm
Petroleum	0.02724– ohm	Chloroform vapor .	0.02725– ohm
Wood alcohol . . .	0.02727– ohm	Wood alcohol and petroleum . . .	0.02727– ohm
Absolute alcohol . .	0.02725 ohm		

After the above measurements were made, the wire was removed from the tube, and was coated electrolytically with copper from a bath of copper sulphate. The copper coating, while adherent to the silver, was very rough and porous, and it was very difficult

to remove any adhering liquid from it. The measurements made with it were more irregular than those which had been made with the wire before coating it with copper, but there was no possibility of confounding the air curve with the curve made when either of the other dielectrics was used. The resistances at 20° in the dielectrics used were as follows: —

SILVER WIRE, S_2, S_1, COATED ELECTROLYTICALLY WITH COPPER.

Dielectric.	Resistance.	Dielectric.	Resistance.
Air	0.02561	Petroleum and wood alcohol	0.02564
Petroleum	0.02562+	Ether vapor	0.02559

It will be seen that not only the arrangement of the dielectrics with regard to their influence upon the conductivity of the wire is the same as before the wire was copper-plated, but that the absolute difference in resistance is almost the same as before. This result was entirely unlooked for on my part.

The following table is intended to show the resistances at corresponding temperatures in the dielectrics used of all the wires above mentioned: —

RESISTANCES.

Dielectrics.	C_1	C_2	C_3	C_4	C_5
Air	0.03375	0.02618	0.02842	0.01915	0.02640+
Petroleum	0.03369	—	—	?	—
Wood alcohol . . .	0.03376	—	—	—	—
Benzine	0.03377	—	—	—	—
Wood alcohol and benzine	0.03380	—	—	—	—
Absolute alcohol . .	0.03381	—	—	0.01919	—
Wood alcohol and petroleum . . .	0.03382	—	—	—	—
Alcohol vapor . . .	0.03377(?)	—	—	—	—
Chloroform vapor . .	0.03380+	—	—	—	—
Burning gas . . .	0.03381	0.02626	0.02842	0.01917	—
Ether vapor . . .	0.03383	—	—	—	0.02646

RESISTANCES (*continued*).

Dielectric.	C_5 second series.	C_6	S_1	S_2	
Air	0.02660	0.17715	0.02722	0.02561	
Petroleum	0.02656	0.17715	0.02724−	0.02562+	
Wood alcohol . . .	—	—	0.02727−	—	
Absolute alcohol . .	—	—	0.02725	—	
Wood alcohol and petroleum . . .	?	—	0.02727	0.02564	
Alcohol vapor . . .	—	—	—	—	
Chloroform vapor . .	—	—	0.02725−	—	
Burning gas . . .	?	0.17716	—	—	
Ether vapor . . .	—	—	0.02719−	0.02559	

Thus far I have offered no explanation of the above-mentioned phenomenon. The only hypothesis which I am able to advance, while it would suggest the probability of a difference in resistance in different dielectrics, would hardly account for the irregularity observed in different wires. It will be noticed, however, that while the irregularity in quantitative results is very great, the variations in resistance with wires of the same material are always in the same direction. This is true of the copper wire before and after having its surface amalgamated, and of the silver wire before and after being copper-plated. In the results already referred to, which have been published by Signors Grimaldi and Platania, the variation observed between the air and petroleum resistances of the copper wires used, while less than some of the variations observed by me, were in the same direction. Since the hammering of a wire or the oiling of its surface may produce such a marked change in the degree to which its conductivity is affected by different dielectrics, it is not strange that different observers, working with different samples of wire, should get very different results; and there is, accordingly, nothing contradictory in Professor Carhart's results and my own.

The only explanation which I am able to offer is based upon the assumption that the passage of a current through a metallic conductor is accomplished by means of disruptive discharges from

molecule to molecule through the intervening ether. These discharges would, like the discharge of a Leyden jar, be of an oscillatory character, although the discharge in one direction would be of much greater intensity than in the opposite; and their rate of oscillation would be affected by the elasticity and density of the intervening ether. While I am not aware that the rate of oscillation of a condenser discharge has been observed to vary in different dielectrics, it is known that the capacity of a condenser does so vary, and this would necessitate a variation in the rate of oscillation of its discharge. In the case of the outer layer of molecules of a wire, the surrounding dielectric would influence the rate of oscillatory discharge between contiguous molecules. If the dielectric should penetrate for some distance into the wire, the rate of discharge of a correspondingly great number of layers of molecules would be affected. That gases, at least, frequently do penetrate for some distance into metals is well known. In the case of some of the dielectrics used by me, something of this character seemed to take place. This was especially noticeable in the case already referred to of the sulphuric ether vapor and the copper wire. If, however, the dielectric should penetrate to a definite depth into the wire, the difference in resistance should be more marked in fine wires than in coarse ones, since a larger proportion of the molecules would have their rate of discharge influenced by the surrounding dielectric. My own results are too irregular to enable me to decide whether this is the case or not, but the variation in resistance observed in the fine wire used by Signors Grimaldi and Platania was much greater in proportion to the cross-section of the wire than in any of the wires used by me.

It seems probable that after a current has been once established in a wire, all the molecules in a given cross-section will receive their charges at the same time, and part with them at the same time. This would necessitate the same rate of discharge for both external and internal molecules. It might, nevertheless, occur that the hastening or retarding of the rate of discharge of the external molecules would affect the rate of all the molecules of the wire.

One conclusion which seems necessarily to follow from this

hypothesis concerning conductivity is that the variation in resistance in different dielectrics should be greater for alternating than for direct currents. I have not, however, been able to verify this conclusion, owing to the difficulty experienced in measuring resistance accurately by means of alternating currents.

In my first paper, I published the results of some attempts to find a relation between the effect of the different dielectrics upon the conductivity of the copper wire surrounded by them and their refractive index for light. In this attempt I was unsuccessful, but scarcely more so than have been the other attempts to establish a relation between the refractive index and the specific inductive capacity. Aside from the general discrepancy observed between the values of these two properties of dielectrics, it seems probable that neither Fresnel's nor MacCullagh's theory of the propagation of light in material bodies will give a complete explanation of the phenomenon, and that the refractive index of a substance will not alone enable one to judge of the elasticity or the density of the ether in that substance.

In a similar manner, if some dielectrics are absorbed more than others by the surface of a conductor, a mere knowledge of the specific inductive capacity of a dielectric will not enable one to foretell its effect upon the conductivity of a wire immersed in it.

I am aware that this hypothesis as to the nature of metallic conductivity leads to certain conclusions not experimentally established as to the nature of the field of force surrounding the current. If such oscillatory discharges as are here presumed take place between the molecules of a conductor, the entire field about the conductor would be filled with Hertzian waves of very short period. Whether the magnetic field about a current can be accounted for on the assumption of the existence of these waves, I do not know. The velocity of electro-magnetic induction could be satisfactorily explained in this way, and it is certain that very rapid ether vibrations of this kind can produce magnetic effects, but whether the magnetic polarity of the field may be accounted for by the greater intensity of the discharges in one direction than in the opposite, I do not know.

STANFORD UNIVERSITY, June 28, 1895.

A STUDY OF THE POLARIZATION OF THE LIGHT EMITTED BY INCANDESCENT SOLID AND LIQUID SURFACES. II.

By R. A. Millikan.

X.

Application of Fresnel's Formulæ for Vitreous Reflection.

THE main object of this research being to determine whether or not polarization by emission could be experimentally proven to be a phenomenon of refraction, Fresnel's laws for reflection and refraction, which have been shown by many experiments to accurately represent the facts, were now applied to the determination of the amounts of polarization which should be produced by single refraction of light passing through the boundary surface between uranium glass and air. In order to apply these laws it is necessary to assume that all of the light emitted by the uranium glass, whether coming *from the surface molecules or from the interior layers*, has undergone the process of refraction — an assumption not contained in Arago's explanation of the cause of the phenomenon.

Taking the intensity of the incident ray as unity, Fresnel's formulæ give for the intensities of the reflected and refracted rays when the incident beam is plane polarized in the plane of incidence,

$$\text{reflected ray} = r_1 = \frac{\sin^2(\alpha-\beta)}{\sin^2(\alpha+\beta)},$$

$$\text{refracted ray} = r_1' = \frac{4\cos^2\alpha\sin^2\beta}{\sin^2(\alpha+\beta)},$$

α being the angle of incidence and β the angle of refraction.

For a ray plane polarized in a plane perpendicular to the plane of incidence

$$\text{reflected ray} = r_2 = \frac{\tan^2(a-\beta)}{\tan^2(a+\beta)},$$

$$\text{refracted ray} = r_2' = \frac{4\cos^2 a \sin^2\beta}{\sin^2(a+\beta)\cos^2(a-\beta)}.$$

Since ordinary light may be considered as composed of two equal plane polarized beams, polarized in planes at right angles to each other, the amount of polarization in a beam of natural light which has undergone single refraction is

$$\frac{\frac{1}{2}r_2' - \frac{1}{2}r_1'}{\frac{1}{2}r_2' + \frac{1}{2}r_1'} = \frac{\frac{1}{\cos^2(a-\beta)} - 1}{\frac{1}{\cos^2(a+\beta)} + 1} = \frac{1-\cos^2(a-\beta)}{1+\cos^2(a+\beta)} = p.$$

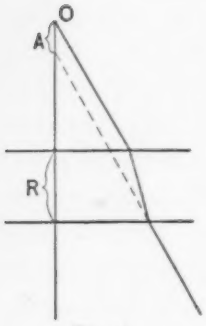


FIG. 4.

The only unknown quantity in this formula is the angle β. In order to determine p for any given angle it was only necessary to determine the index of refraction of the uranium glass.

The glass being of considerable thickness, the microscope method was the one best adapted to this determination. d being the thickness of the glass, and a the change in focus due to the introduction of the glass between the object O and the objective, the index u is given by the formula (see Kohlrausch, *Praktische Physik*, p. 151),

$$u = \frac{d}{d-a} = \frac{326\text{ mm.}}{326\text{ mm.} - 110\text{ mm.}} = 1.51 = \frac{\sin a}{\sin\beta}.$$

The substitution of the various values of β, thus found, in the formula for p gave the following values:—

a	β	p	p (observed)
87° 30′	41° 25′	0.351	0.358
85°	41° 17′	0.315	0.293
80°	40° 42′	0.251	0.245
75°	39° 46′	0.206	0.191
70°	38° 29′	0.153	0.139
65°	36° 53′	0.125	0.098
50°	30° 29′	0.058	0.039

The correspondence between the results given by experiment and those given by this calculation from Fresnel's formulæ was unexpected. The experiments were completed more than a month before any calculations were made, so that I had no idea at the time of making the experiments what would be the nature of the results given by calculation.

The differences between the two sets of values are hardly greater than the possible errors of observation. The differences at 65° and 50° are quite large, but might have been due to the lack of perfect uniformity in the luminous surfaces. On the whole, the agreement between the two sets of results indicates strongly that in the case of uranium glass, at least, the phenomenon is one of simple refraction at the surface; but that the WHOLE *of the emitted light undergoes the refraction process.*

XI.

Experiments upon Platinum.

It is evident that no such comparisons as those just made for uranium glass could be made for the case of incandescent metals, unless, in the first place, the surface experimented upon could be assumed to be a perfectly definite, non-diffusing surface. The chief source of difficulty in the work upon platinum was to fulfil this condition.

It was found, after considerable work had been done upon platinum, that continual heating roughened the surface to a slight degree, and changed the amount of polarization. The results of several sets of laborious observations upon platinum were discarded altogether, because they were found to be erroneous from this cause. However, the change is so gradual that a well-polished platinum surface may be heated to incandescence for several minutes without showing any perceptible change in character. The rapidity of the change could be delicately observed by viewing the surface at a large angle of incidence by means of the polarimeter. For a period of two or three minutes no change was perceptible in the equality of the images, but for much longer periods of heating the slow blistering of the surface began

to be manifest in the disturbance of the equality of the images. Hence, in order to avoid this error, the surface of the platinum was carefully polished with rouge after every set of readings for a given angle.

A second slight source of error in the observations upon platinum was the lack of exact horizontality in the surface examined. The attempt was made to avoid this error by rotating the instrument through 90° according to the suggestion of Cornu. This brought the extraordinary image either above or below the ordinary; hence, when the angle of emergence was very large, the two images corresponded to points on the surface at a considerable distance from each other, as shown in the figure.

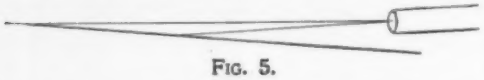


FIG. 5.

This introduced the likelihood of a much greater error than that due to a slight error in horizontality. The incandescent platinum was therefore rendered as nearly horizontal as possible by comparison with carefully leveled reference planes placed in the immediate vicinity. The adjustment could thus be easily made to within one degree.

In all of the following experiments sheets of rolled platinum 0.06 mm. in thickness were heated to a white heat by means of a Bunsen burner, care being taken to prevent light from any other sources from vitiating the results. The observations are here given in full.

80°		70°		60°	
Left.	**Right.**	**Left.**	**Right.**	**Left.**	**Right.**
23.1	12.0	30.2	20.7	36.9	24.5
21.4	11.5	32.2	21.0	36.8	26.1
22.5	11.3	32.1	21.2	35.0	25.0
22.3	10.9	31.0	20.5	37.0	25.4
23.2	11.4	32.1	21.3	35.6	25.5
				35.5	25.4
22.5	11.42	31.52	20.94	36.05	25.2
$2w = 34°.0$		$2w = 52°.4$		$2w = 61°.25$	
$p = 0.829$		$p = 0.610$		$p = 0.481$	

50°		40°		30°	
Left.	Right.	Left.	Right.	Left.	Right.
40.0	29.7	45.6	35.0	47.6	38.0
39.1	29.0	45.5	33.3	47.5	36.3
41.2	29.3	44.4	34.2	47.2	36.2
39.4	28.5	43.8	34.6	47.8	37.1
41.0	29.6	44.4	34.5	48.0	36.0
40.14	29.2	44.74	34.3	47.62	36.72
$2w = 69°.34$		$2w = 79°.0$		$2w = 84°.34$	
$p = 0.349$		$p = 0.191$		$p = 0.099$	

XII.

Application of Cauchy's Formulæ for Metallic Reflection to the Case of Platinum.

Fresnel's formulæ for reflection rest upon the hypothesis that the time required in the process of reflection is infinitesimal in comparison with a wave period, and hence that the phase of vibration of the reflected ray is either the same as that of the incident ray, or else differs from it by the quantity π. It follows from this assumption that the reflected ray is plane polarized, if the incident ray is plane polarized.

When the reflection takes place at the boundary surface between air and a metal, experiment shows this assumption to be incorrect, and hence Fresnel's formulæ become inapplicable.

If the phenomenon here considered be due to reflection, the laws for reflection which apply to the boundary surface between platinum and air are, of course, the laws to apply to the determination of the amounts of polarization which ought to be caused by a single refraction at this boundary.

The application of Fresnel's laws of vitreous reflection requires, as has been seen, the determination of but one constant, the index of refraction, or the ratio of the velocities of propagation of light in the two media. Cauchy extended these laws so as to cover the case of metallic reflection by introducing another constant which he calls the coefficient of extinction. The con-

stant corresponding to the index of refraction is, as in the case of transparent bodies, the tangent of the angle of maximum polarization. The coefficient of extinction is a constant depending upon the opacity of the body, and is found from the ratio between the amplitudes, after reflection, of two equal beams polarized respectively perpendicular and parallel to the plane of incidence, and reflected at the angle of maximum polarization. This ratio is evidently the tangent of the azimuth of re-established plane polarization, when the incident beam is polarized in a plane making an angle of 45° with the plane of incidence, plane polarization being re-established after reflection by means of a quarter-wave plate or a Babinet compensator.

This angle may be determined by experiment. Thus the two constants of metallic reflection are, (1) the angle of maximum polarization, and (2) the azimuth of re-established plane polarization at this angle. According to the theory of Cauchy, these two consonants being known, the intensity of a beam reflected at any angle may be calculated.

The complete explanation of Cauchy's theory and the deduction of Cauchy's formulæ were given by Eisenlohr in 1858. (See *Pogg. Ann.* 104, p. 368.)

The final forms of the formulæ given by Eisenlohr are

$$K^2=\tan\,(f-45°),\quad K'^2=\tan\,(g-45°), \tag{1}$$

in which K^2 is the intensity of the reflected beam when the incident beam is polarized in the plane of incidence, K'^2 the intensity when the incident beam is polarized in the plane perpendicular to the plane of incidence, and f and g are variables given by the equations

$$\left.\begin{aligned}\cot f&=\cos\,(e+u)\,\sin\left(2\,\arctan\frac{c\theta}{\cos a}\right)\\ \cot g&=\cos\,(e-u)\,\sin\left(2\,\arctan\frac{\cos a}{c\theta}\right)\end{aligned}\right\}, \tag{2}$$

in which u and e are variables determined by the relations

$$\left.\begin{aligned}\cot\,(2u+e)&=\cot e\,\cos\left(2\,\arctan\frac{\sin a}{\theta}\right)\\ c^2&=\frac{\sin 2e}{\sin\,(2u+2e)}\end{aligned}\right\}, \tag{3}$$

in which a is the angle of incidence and e and θ are given by the final formulæ

$$\left.\begin{aligned} \sin 2e &= \tan^2 A \sin (4H - 2e) \\ \theta &= \sin A \sqrt{\frac{\sin 4H}{\sin (4H - 2e)}} \end{aligned}\right\}. \qquad (4)$$

A is the angle of maximum polarization, called the "principal angle of incidence," and H is the azimuth of re-established plane polarization when the incident beam is polarized in the azimuth 45°. H is called the "prime azimuth." The forms here given for e and θ are due to Jochman (see *Pogg. Ann.* CXXXVI., p. 856). These formulæ, first published by Cauchy in 1839, were shown by Jamin, by an elaborate series of measurements, to very closely represent the facts of reflection from metallic surfaces. The prime angles of incidence and the prime azimuths for all the common metals and for the different Fraunhofer lines were determined by Quincke in 1874 (see *Phil. Mag.* XLVII., p. 221).

Now, in order to apply these formulæ to calculations similar to those which have already been made with Fresnel's formulæ upon uranium glass, it was necessary to assume, as before, that the whole of the light emitted had undergone refraction, and it was also necessary to know the two optical constants for platinum at the temperature of incandescence. These constants could not be determined. However, in a number of experiments made by W. R. Grove (see *Phil. Mag.* (4) 17, p. 177) upon the reflection of light from incandescent platinum, he was unable to detect any change in the reflecting properties of the platinum due to the fact of incandescence. Plane polarized light being reflected from the cold surface, the plane of polarization of the reflected beam was not affected by heating the platinum to the incandescent temperature. These experiments were not performed with delicate apparatus, yet they give reason to assume that the optical constants of platinum are not greatly altered by temperature.

Assuming, then, the values of A and H given by Quincke for the sodium line, the calculations of the amount of polarization in the emitted beam were made for all the angles of emergence

for which experiments had been made. These calculations were made as follows: —

Quincke's values for the D line are

$$A=77^\circ\ 8',\ H=32^\circ\ 46'.$$

Formulæ (4) give

$$e=64^\circ\ 22',\ \log\theta=0.62276.$$

Then for $a=80^\circ$ formulæ (3), (2), and (1), give

$$K^2=0.9348,\ K'^2=0.4013.$$

Assuming now the incident beam to have had an intensity unity, the emitted beam polarized in the plane of emergence would have an intensity

$$1-K^2=0.0652$$

and the beam polarized in the plane perpendicular to the plane of emergence an intensity

$$1-K'^2=0.5987.$$

Therefore the degree of polarization $=p=\dfrac{0.5987-0.0652}{0.5987+0.0652}=0.834.$

The complete results of the calculations for the platinum are as follows: —

a	K^2	K'^2	$1-K^2$	$1-K'^2$	p
80°	9348	4013	0652	5987	0.834
70°	8757	4044	1243	5957	0.655
60°	8234	4813	1766	5187	0.492
50°	7782	5483	2218	4517	0.341
40°	7409	5981	2591	4019	0.216
30°	7115	6330	2885	3670	0.117

Considering the number of assumptions which have been made, the correspondence between these quantities and those given by experiment is altogether remarkable, and points with as much certainty as the work upon uranium glass to the conclusion that the phenomenon is simply one of refraction.

XIII.

Difference in Color of Images.

In the course of these observations upon platinum, another at first unaccountable phenomenon was noticed. At large angles of emergence the color in the two images was notably different. The image corresponding to the component polarized perpendicular to the plane of emergence was markedly redder than the other.

If we assume the phenomenon to be due to reflection and refraction, this appearance is readily explained by a reference to Quincke's values for the angles of maximum polarization for the different Fraunhofer lines. This angle for the line *C*, Quincke gives as 78° 28′, and for the line *G* his value is 73° 39′. Now if a is the amplitude of vibration in the reflected ray when the incident beam is polarized in the plane of incidence, and a' the amplitude when the plane of polarization of the incident beam is perpendicular to the plane of incidence, the angle of maximum polarization will be reached when $\frac{a^2-a'^2}{a^2+a'^2}$ is a maximum, *i.e.* when $\frac{a'}{a}$ is a minimum. The experiments of Jamin show that this angle coincides, at least very nearly, with the angle for which a' is a minimum; a conclusion which one would expect without the aid of experiment. Hence the angle 78° 28′ is that angle for which the component of the *reflected* vibration parallel to the plane of incidence is a minimum for the case of *red* light, and the component of the *emitted* vibration in the same plane is a maximum. On the other hand, the angle of maximum emission of *violet* light in this plane occurred at 73° 39′. Accordingly, it is evident that red light predominates in the beam emitted at the angle 78° 28′, and violet in the beam emitted at 73° 39′. The approximate ratio between the two colors for any angle is shown in Fig. 6. It is evident that light emitted at any angle larger than 75° will be predominantly red. At the same time, the shape of the curves accounts for the lack of any noticeable predominance of violet in the neighborhood of 73° 39′.

The figure shows the curves of intensities of the reflected com-

ponents of vibration parallel to the plane of incidence as roughly plotted from the values given above. The lines mD, m_1D_1, etc., represent the intensities of the emitted red vibrations in this plane for various angles; while the lines nD, n_1D_1, etc., represent the intensities of the emitted violet vibrations for the same incidences. The lines nM, n_1M_1, etc., are the measure of predominance of red over violet, or *vice versa*. The steepness of the curves at points corresponding to angles greater than the angle of maximum polar-

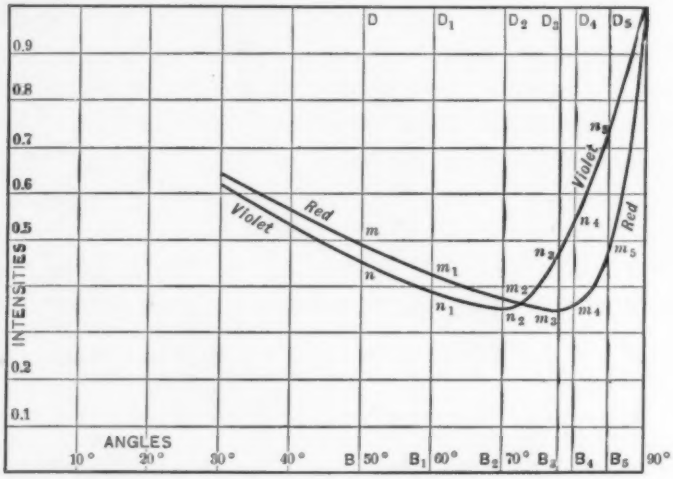


FIG. 6.

ization, and the lack of steepness at points corresponding to angles less than the angle of maximum polarization are evidently the causes of the predominance of red at large angles, and the lack of marked predominance of violet at any angles. This characteristic of the curves follows from the fact that the points of maximum polarization correspond to very large angles.

XIV.

Experiments upon Silver.

Owing to the great kindness of Mr. Herbert G. Torrey, Assayer of the U. S. Assay Office, I was next able to make a series of observations upon molten silver. These experiments were the

most satisfactory of any which were made in the course of the research. All of the sources of error which had existed in preceding cases were here eliminated. The surface was perfectly defined, it was accurately horizontal, and there were no variations in intensity from point to point. The results of the experiments are given in full.

30°		35°		40°		45°	
Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.
46.0	36.0	44.5	33.4	43.3	31.7	42.0	30.5
47.2	36.0	45.0	33.7	42.8	32.5	42.0	31.0
46.5	35.0	44.5	34.0	43.3	32.0	41.1	31.3
46.3	35.0	45.0	34.0	43.5	32.0	42.5	30.5
46.5	35.5						
46.5	35.5	44.75	33.8	43.72	32.05	41.9	30.8
$2w = 0°.82$		$2w = 78°.55$		$2w = 75°.27$		$2w = 72°.7$	
$p = 0.139$		$p = 0.189$		$p = 0.254$		$p = 0.297$	

50°		55°		60°		65°	
Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.
40.6	28.5	38.0	27.0	34.7	24.1	32.0	21.0
40.0	29.0	38.3	27.5	35.0	24.0	32.5	21.5
40.0	29.8	37.5	27.1	34.0	24.1	32.6	21.0
40.5	29.1	37.5	26.3	35.4	23.8	32.5	20.5
40.0	28.8	38.0	26.5	35.7	24.0	32.5	21.5
				34.5	24.0		
40.2	29.04	37.8	26.8	34.9	24.0	32.4	21.3
$2w = 69°.24$		$2w = 64°.6$		$2w = 58°.9$		$2w = 53°.7$	
$p = 0.354$		$p = 0.429$		$p = 0.517$		$p = 0.592$	

70°		75°		80°			
Left.	Right.	Left.	Right.	Left.	Right.		
30.0	20.0	27.0	16.5	24.1	13.5		
30.0	19.5	27.0	16.5	24.1	13.5		
29.6	19.9	27.0	16.5	24.2	14.0		
30.0	20.4	26.5	16.1	23.9	13.7		
30.5	19.5	27.0	16.4	24.0	13.9		
		26.5	15.5				
30.0	19.9	26.8	16.25	24.1	13.7		
$2w = 49°.9$		$2w = 43°.05$		$2w = 37°.8$			
$p = 0.644$		$p = 0.731$		$p = 0.789$			

All of the previous observations had been subject to errors of unknown magnitude, aside from the errors of observation; and the results, while agreeing very closely with the calculated values for some angles, differed from them by considerable amounts at others. For example, the agreement for platinum at 80° was very close, while at 70° the difference was as large as .045. Similarly for uranium glass, the difference at 50° and 65° was quite large. Hence I did not consider the results given by the experiments upon uranium glass and platinum altogether trustworthy as accurate quantitative measurements. The experiments upon silver, however, were free from all possible error so far as I was able to discover, except the observational error. Mention has already been made of the fact that Violle had previously made a number of determinations of the same general nature upon silver. His results do not agree very closely with those given above, being uniformly larger. I am altogether unable to account for the uniformity in the excess of his values over those given by these experiments. His results are here inserted for the sake of comparison.

Angle.	Violle.	Millikan.
30	0.168	0.139
50	0.383	0.354
60	0.546	0.517
65	0.630	0.592
70	0.708	0.644
75	0.770	0.731
80	0.826	0.789

The application of Cauchy's formulæ to silver was made in the same way as in the case of platinum. For polished silver Quincke gives for the *D* line,

Prime angle of incidence, $A=72°\ 10'$,
Prime azimuth, $H=41°\ 40'$.
Formulæ (4) give $e=82°\ 34'.3$, $\log\theta=0.4480$.

Formulæ (3), (2), and (1) then give

a	K^2	K'^2	$1-K^2$	$1-K'^2$	p
80	0.9735	0.8037	0.0265	0.1963	0.762
75	0.9606	0.7628	0.0394	0.2372	0.716
70	0.9482	0.7510	0.0518	0.2489	0.655
65	0.9361	0.7540	0.0639	0.2460	0.588
60	0.9250	0.7632	0.0750	0.2368	0.519
55	0.9136	0.7740	0.0864	0.2260	0.446
50	0.9033	0.7869	0.0967	0.2131	0.376
45	0.8937	0.7985	0.1063	0.2015	0.309
40	0.8847	0.8093	0.1153	0.1907	0.246
35	0.8767	0.8187	0.1233	0.1813	0.190
30	0.8695	0.8268	0.1305	0.1732	0.140

It will be noticed that the agreement between these quantities p and those given by my experiments is closer than for either the platinum or the uranium glass; the largest difference being at 80°, where it amounts to 0.027.

XV.

Experiments upon Gold and Iron.

Through the kindness again of Mr. Torrey and the Superintendent of the Subtreasury, I was permitted to make observations upon a pot of molten gold; but accuracy of work was impossible on account of (1) the rapidity with which I was obliged to work; (2) the lack of quiescence of the liquid surface; (3) the impossibility of excluding other light from the surface; and (4) the rapidity of oxidization of the molten gold. The results were therefore altogether untrustworthy as quantitative measurements. Hence no attempt was made to compare them with results given

by Cauchy's formulæ. Molten iron was also made the subject of similar observations with equally unsatisfactory results.

XVI.

Discussion of Results.

The comparisons made between experimental determinations and calculated values are condensed in the following tables:—

Uranium glass.				Platinum.				Silver.			
Angle.	p. obs.	p. calc.	Difference.	Angle.	p. obs.	p. calc.	Difference.	Angle.	p. obs.	p. calc.	Difference.
87½	0.358	0.351	0.007	80	0.829	0.834	−0.005	80	0.789	0.762	+0.027
85	0.293	0.315	−0.022	70	0.610	0.655	−0.045	75	0.731	0.716	+0.015
80	0.245	0.251	−0.006	60	0.481	0.492	−0.011	70	0.644	0.655	−0.011
75	0.191	0.206	−0.015	50	0.349	0.341	+0.008	65	0.592	0.588	+0.004
70	0.139	0.153	−0.014	40	0.191	0.216	−0.025	60	0.517	0.519	−0.002
65	0.098	0.125	−0.027	30	0.099	0.117	−0.018	55	0.429	0.446	−0.017
50	0.039	0.058	−0.019					50	0.354	0.376	−0.022
								45	0.297	0.309	−0.012
								40	0.254	0.246	+0.008
								35	9.189	0.190	−0.001
								30	0.139	0.140	−0.001

In view of the general agreement between the observed and calculated values, and in view of the further fact of the coloration of the images at large angles, so beautifully accounted for by the reflection theory, it may be considered that the phenomenon of polarization of light by emission has thus been quantitatively proven to be a phenomenon of reflection and refraction.

It will be remembered that the apparently insuperable objection to the explanation which Arago offered was that that explanation attributed to all of the surface molecules the property of emitting natural light, and gave as the entire cause of the polarization, the refraction of light which works its way up from a certain depth beneath the surface.

The above calculations were all made upon the assumption that *all* of the light emitted by the glowing body had under-

gone a refraction. Considering the closeness of agreement between the calculated and observed values, it is difficult to escape the conclusion that this assumption is correct, and that *no* particles whatever of the incandescent solid send out into the air natural light, save in the case in which the angle of emergence is zero. This simply means that all of the particles of the light-emitting body, *including* the so-called surface layers, lie within the denser medium, and beneath the plane at which reflection and refraction take place. This relieves the refraction theory of the causes of the phenomenon of its greatest difficulty: viz., the difficulty of conceiving that, in the case of an exceedingly opaque body like platinum, the uppermost molecules send out but a very small proportion of the whole light emitted. If we follow the explanation of Arago and Verdet, we are obliged by the results of this research to conclude that the emitted light originates almost entirely in molecules other than those of the uppermost layer. On the contrary, it seems much more reasonable to assume that in the case of such a body as platinum the light emitted is due mainly to this topmost layer, but that the reflection process takes place entirely *above* the platinum.

Quincke has shown that when light from an external source is reflected at the surface of a metal, the reflection does not take place in the geometrical plane between the two media, but rather takes place in the metal itself, the vibration penetrating for a certain depth into the denser medium. The converse is also doubtless true that the vibration originating in the metal is not reflected instantaneously at the surface of the rarer medium, but is reflected in the layer of air of finite thickness which borders upon the metal. Thus all light originating in the platinum, whether in the surface layer or the sub-surface layers, must undergo the process of reflection and refraction before it can emerge into the air.

Lastly, the calculated values were all obtained under the assumption that the optical constants of the metals are the same for high temperatures as for low; that is, that the reflecting properties of an incandescent metallic surface are precisely the same as the reflecting properties of a cold metallic surface. The closeness of agreement between the results given by this assumption and the

facts as determined by experiment seems to warrant the conclusion, that the change in the optical properties of metals due to incandescence is *exceedingly slight;* a conclusion to which the somewhat inexact experiments of Grove upon the reflecting properties of incandescent platinum would also lead.

The results of the investigation may therefore be summarized as follows:—

(1) Experiments upon polarization by emission have been extended to a wider range of substances than had previously been investigated, and these substances have been classified with reference to their power of producing the phenomenon.

(2) The polarization of the light emitted by flourescent bodies has been, I believe for the first time, observed and measured.

(3) The difference in the color of the principal components of the light emanating at large angles from white-hot metals has been observed and explained. So far as I am able to discover, this fact had not been before known.

(4) Some experimental ground has been given for the conclusion that all light originating in an incandescent body, whether in the surface molecules or in the interior molecules, must suffer a reflection and refraction before actual emergence.

(5) The reflecting properties of metals have been shown to be but little, if at all, affected by the fact of incandescence.

(6) The phenomenon of polarization of light by emission has been shown conclusively to be a phenomenon of refraction, first, by the closeness of agreement between a large number of experimental and calculated quantities, and second, by the fact of the difference in the color of the images at large angles of emergence which finds complete explanation in the refraction theory.

In conclusion, I will add that this investigation was suggested to me by Professor Rood, and I wish here to express my thanks to him, and to Professor Hallock, and also to Professor A. A. Michelson, of Chicago University, for aid furnished during its progress. I am also under obligations to Herbert G. Torrey, Assayer of the U. S. Assay Office, who most kindly placed at my disposal large masses of molten gold and silver.

PHYSICAL LABORATORY OF COLUMBIA COLLEGE.

ON TERNARY MIXTURES. III.

By Wilder D. Bancroft.

In addition to the results given in Tables XIX.–XXXI.[1], Pfeiffer made a few measurements on amylalcohol, monochlor-, dichlor-, and trichloracetic ester in the presence of alcohol and water. The solubility of amylalcohol in water is given by Roscoe and Schorlemmer as two parts in a hundred, and I have used this value. I could find no data whatsoever in regard to the chloracetic esters, so I have calculated the values on the false assumption that they are non-miscible with water. The effect of this error is seen very markedly in the case of the monochloraceticester, which is undoubtedly the most soluble of the three. I give these tables in spite of the known inaccuracy, because the absolute values of the constants are, for the time being, of little value, whereas it is essential to show that the same general law covers all substances and that the substitution of chlorine for hydrogen does not affect the action of the Mass Law. The coincidence of the three chloraceticesters having the same exponential factor is probably only superficial, as the correction for the solubilities would alter the exponential factor somewhat.

Table XXXII.

$y = 3$ c.c. Amylalcohol; x = c.c. Water; z = c.c. Alcohol.
Formula $x(y = 0.02\,x)^{0.4}/z^{1.4} = C$; log $C = 0.100$. Temp. 9.1°.

	x.		
z.	Calc.	Found.	log C.
3	3.81	3.21	—
6	10.26	10.35	0.104
9	18.53	18.34	0.095
12	28.45	27.47	0.085
15	40.85	41.25	0.104
			0.097

[1] Tables XXIII.–XXXI. are given at the close of this article.

TABLE XXXIII.

$y = 3$ c.c. Amylalcohol ; x = c.c. Water ; z = c.c. Alcohol.
Formula $x\,(y - 0.02\,x)^{0.4}/z^{1.4} = C$; log $C = 0.112$. Temp. 19.2°.

	x.		
z.	Calc.	Found.	log C.
3	3.93	3.50	—
6	10.55	10.80	0.122
9	19.10	19.10	0.112
12	30.05	29.15	0.099
15	42.30	43.15	0.121
			0.114

TABLE XXXIV.

$y = 3$ c.c. Monochloraceticester ; x = c.c. Water ; z = c.c. Alcohol.
Formula $xy^{0.43}/z^{1.43} = C$; log $C = \bar{1}.700$.

	x.		
z.	Calc.	Found.	log C.
3	1.54	1.32	$\bar{1}.644$
6	4.05	4.01	$\bar{1}.695$
9	7.23	7.30	$\bar{1}.705$
12	10.91	10.78	$\bar{1}.695$
15	15.04	16.16	$\bar{1}.731$
18	19.50	22.16	$\bar{1}.756$
21	24.33	28.74	$\bar{1}.772$
			$\bar{1}.714$

TABLE XXXV.

$y = 3$ c.c. Dichloraceticester ; x = c.c. Water ; z = c.c. Alcohol.
Formula $xy^{0.43}/z^{1.43} = C$; log $C = \bar{1}.479$.

	x.		
z.	Calc.	Found.	log C.
3	0.90	0.90	$\bar{1}.477$
6	2.44	2.45	$\bar{1}.481$
9	4.35	4.33	$\bar{1}.477$
12	6.54	6.60	$\bar{1}.482$
15	9.04	9.20	$\bar{1}.487$
			$\bar{1}.481$

TABLE XXXVI.

$y = 3$ c.c. Trichloraceticester; x = c.c. Water; z = c.c. Alcohol.

Formula $xy^{0.43}/z^{1.43} = C$; $\log C = \bar{1}.336$.

	x.		
z.	Calc.	Found.	log C.
3	0.65	0.65	$\bar{1}.336$
6	1.76	1.80	$\bar{1}.347$
9	3.13	3.02	$\bar{1}.321$
12	4.72	4.50	$\bar{1}.315$
15	6.50	6.50	$\bar{1}.336$
			$\bar{1}.331$

Tables XIX.–XXXI. furnish a striking confirmation of the way in which the Mass Law applies to this class of phenomena; while some of the results are not as satisfactory, perhaps, as I should like, there are some, notably those with propylbutyrate, where the agreement between the observed and the calculated values is something marvelous, though it is unfortunate that the solubility of propylbutyrate in water has never been determined experimentally.

As it might be thought a mere assumption that the first measurements in several series were determinations of another equilibrium, namely, of a saturated solution from which water or ester precipitated water, I have made a few measurements with the few esters I had on hand. The object of these measurements was to show that the change from one equilibrium to another did come at the point shown by Pfeiffer's results, and to make sure that the variations in Pfeiffer's data were due to experimental error. On this account I have made no measurements on the end curves, where water and where ester are part solvents, and in the case of ethylisovalerate I have measured only one series. The results are given in Tables XXXVII.–XXXIX.

TABLE XXXVII.

x = c.c. H_2O; y = c.c. Ethylisovalerate; 5 c.c. Alcohol. Temp. 20°.

Formula $(x - 0.004\,y)^n\,(y - 0.002\,x)/z^{n+1} = C$; $n = 2.45$; $\log C = \bar{1}.149$.

Water.		Et. Val.		
Calc.	Found.	Calc.	Found.	log C.
9.98	10.00	0.15	0.15	$\bar{1}.152$
8.05	8.00	0.24	0.23	$\bar{1}.142$
6.01	6.00	0.46	0.46	$\bar{1}.147$
4.99	5.00	0.72	0.72	$\bar{1}.152$
4.00	4.00	1.23	1.23	$\bar{1}.149$
				$\bar{1}.148$

TABLE XXXVIII.

x = c.c. H_2O; y = c.c. Ethylbutyrate; 5 c.c. Alcohol. Temp. 20°.

Formula $(x - 0.005\,y)^{n_1}\,(y - 0.008\,x)/z^{n_1+1} = C_1$; $n_1 = 2.44$; $\log C_1 = \bar{1}.449$.

x.		y.		
Calc.	Found.	Calc.	Found.	log C_1.
9.99	10.00	0.34	0.34	$\bar{1}.450$
8.01	8.00	0.51	0.51	$\bar{1}.447$
5.97	6.00	0.95	0.96	$\bar{1}.453$
5.01	5.00	1.45	1.44	$\bar{1}.447$
3.99	4.00	2.46	2.47	$\bar{1}.451$
				$\bar{1}.449$

Formula $(x - 0.005\,y)^{n_2}\,(y - 0.008\,x)/z^{n_2+1} = C_2$; $n_2 = 1.20$; $\log C_2 = \bar{1}.623$.

				log C_2.
2.96	2.96	3.99	4.00	$\bar{1}.624$
2.46	2.48	4.94	5.00	$\bar{1}.628$
2.12	2.10	6.07	6.00	$\bar{1}.618$
				$\bar{1}.623$

TABLE XXXIX.

x = c.c. Water; y = c.c. Isoamylacetate; 5 c.c. Alcohol. Temp. 20°.

Formula $(x - 0.012y)^{n_1}(y - 0.002x)/z^{n_1+1} = C_1$; $n_1 = 3.50$; $\log C_1 = \bar{1}.414$.

x.		y.		
Calc.	Found.	Calc.	Found.	$\log C_1$.
7.00	7.00	0.41	0.41	$\bar{1}.414$
6.00	6.00	0.70	0.70	$\bar{1}.414$
5.01	5.00	1.32	1.31	$\bar{1}.411$
				$\bar{1}.413$

Formula $(x - 0.012y)^{n_2}(y - 0.002x)/z^{n_2+1} = C_2$; $n_2 = 1.50$; $\log C_2 = \bar{1}.559$.

				$\log C_2$.
3.62	3.61	3.00	3.00	$\bar{1}.558$
3.00	3.01	3.99	4.00	$\bar{1}.560$
2.60	2.60	5.00	5.00	$\bar{1}.559$
				$\bar{1}.559$

Although Pfeiffer does not say so, his amylacetate and ethylvalerate are unquestionably iso- and not the normal compounds. We can now take up the results given in Tables XXXVII.–XXXIX. and see how satisfactorily they fulfil their object. Ethylbutyrate and amylacetate show the change from one equilibrium to the other at the same point that Pfeiffer found. The ethylbutyrate and ethylisovalerate mixtures are perfectly regular at concentrations beyond those used by Pfeiffer, and the isoamylacetate is normal throughout both in Pfeiffer's work and in mine, so that the variations in Tables XXIX.–XXXI. are due to experimental error. The agreement in results between the two sets is shown in Table XL., where I give in the first column the value of the exponential factor $n+1$ from the formula

$$(x - s_1 y)(y - s_2 x)^n / z^{n+1} = C,$$

and in the second column the values for the simplified integration constant $\log K$.

TABLE XL.

Ester.		$n+1$.	log K.
Ethylisovalerate	Pfeiffer	1.40	$\bar{1}$.773
Ethylisovalerate	W. D. B.	1.41	$\bar{1}$.754
Ethylbutyrate	Pfeiffer	1.41	$\bar{1}$.847
Ethylbutyrate	W. D. B.	1.41	$\bar{1}$.840
Isoamylacetate	Pfeiffer	1.294	$\bar{1}$.893
Isoamylacetate	W. D. B.	1.286	$\bar{1}$.870

As will be seen, the values of $n+1$ are identical, the values for log K, though very close, are not quite the same. This may be due to inaccuracies in the work, but I am more inclined to attribute it to differences in temperature. It is not known at what temperature Pfeiffer worked, and it would take only a slight difference to account for the variation. In Table XLI. I have tabulated the $n+1$ values from Pfeiffer's results, together with log C and log K.

TABLE XLI.

Ester.	$n+1$.	log C.	log K.
Methylisovalerate	1.37	$\bar{1}$.807	$\bar{1}$.859
Ethylisovalerate	1.40	$\bar{1}$.682	$\bar{1}$.773
Ethylisovalerate [1]	1.41	$\bar{1}$.653	$\bar{1}$.754
Methylbutyrate	1.52	$\bar{1}$.888	$\bar{1}$.926
Ethylbutyrate	1.41	$\bar{1}$.785	$\bar{1}$.847
Ethylbutyrate [1]	1.41	$\bar{1}$.774	$\bar{1}$.840
Propylbutyrate	1.378	$\bar{1}$.651	$\bar{1}$.747
Ethylpropionate	1.39	$\bar{1}$.931	$\bar{1}$.878
Propylpropionate	1.45	$\bar{1}$.733	$\bar{1}$.816
Ethylacetate [1]	1.555	—	—
Propylacetate	1.23	0.166	0.135
Butylacetate	1.30	$\bar{1}$.912	$\bar{1}$.932
Isoamylacetate	1.294	$\bar{1}$.861	$\bar{1}$.893
Isoamylacetate [1]	1.286	$\bar{1}$.832	$\bar{1}$.870
Propylformiate	1.38	$\bar{1}$.967	$\bar{1}$.976
Butylformiate	1.333	0.057	0.043
Isoamylformiate	1.35	$\bar{1}$.808	$\bar{1}$.858

[1] My own measurements.

The first thing that strikes one about this table is the way in which so many of the $n+1$ values approximate to 1.40. Why this should be so is entirely unknown. In the log K values we notice that, for the same acid, increasing the carbon atoms in the alcohol radical diminishes the constant. There is only one exception to this, butylformiate, and here the possible error is very large. It looks also as if the constants might be additive, being made up of one factor for the alcohol and another for the acid radical; but the experimental data are too insufficient to justify this hypothesis. It is very much to be hoped that some one will make a careful series of experiments to settle this point.

Formula II. was deduced for the case when the reacting weights of the substances in equilibrium are not functions of the concentration. The measurements of Pfeiffer and myself show that, with the possible exception of the chloroform-water-acetone series, this condition has been satisfied in all the cases studied, though the experiments extended over a wide range of concentrations. This is in flat contradiction with the determinations of the reacting weights by the boiling-point and freezing-point methods. These methods give accurate results only for very dilute solutions, and even then only for certain solutes in certain solvents. To explain the variations, we are forced to assume "double molecules" in some cases, polymerization with increasing concentration in practically all cases, and "variations from the gas laws." I have brought together a large series of measurements in which there is no sign of any of these things. I see only two possible hypotheses to account for this discrepancy: first, to enunciate a new and most interesting law, to wit, presence of a third substance prevents "polymerization" and "variations from the gas laws"; second, the formula for the change of vapor pressure with the concentration is incorrect. The first hypothesis seems to me out of the question, and there remains only the second. It is a bold thing to question so universally accepted a formula, but I feel convinced that it is not right, and that equal reacting weights of different substances do not produce the same change of vapor pressure. I think that the mistake in the past lay in assuming that the work done in compressing a dissolved substance from the volume V_1 to

the volume V_2 by means of a semipermeable piston is equal to $\int p\,dv$ between those limits, irrespective of the nature of solute and solvent. I have already collected some experimental evidence in favor of this view, and I hope before long to be able to establish my point.

The facts brought out in this paper throw light on a research by Abegg[1] carried out under the direction of Arrhenius. Abegg let alcohol diffuse into a salt solution and found, to his surprise, that the salt, instead of remaining equally divided throughout the liquid, diffused somewhat into the part not yet reached by the alcohol. He concludes that this extraordinary behavior can only be accounted for on the assumption that alcohol increases the osmotic pressure of a dissolved salt. What happens is very simple. When the alcohol has diffused only a little way, one may consider the solution as composed of two parts, one containing a large amount of alcohol, the other very little. The dissolved substance, being in this case less soluble in the first layer than in the second, diffuses into the second only to go back again as the alcohol becomes more evenly divided throughout the liquid. Except that the part containing much alcohol and little water merges insensibly into the part containing much water and little alcohol, and is not in equilibrium with it, the case does not differ from two layers formed by ether and water, where it is well known that the concentration of a third substance is not the same in the two layers. The effect of the alcohol is not, as Abegg assumes, to increase the osmotic pressure of the solute, but to diminish its solubility in that portion of the liquid. If, instead of taking salts which were only slightly soluble in alcohol, Abegg had let water diffuse into water containing in solution some substance very soluble in alcohol, slightly soluble in water, he would have observed the opposite effect, and the dissolved substance would have diffused partially into the layer rich in alcohol.

Another line of reasoning which is not quite defensible is that taken by Wildermann,[2] in his paper, "Ueber cyclische Gleichgewichte." His train of thought is something as follows: Suppose he has a system of three phases, bromine, a solution of bromine

[1] Zeitschr. f. ph. Chem., XI. 248. 1893. [2] Ibid., XI. 407.

in water, and the vapor of bromine and water, it being assumed that the amount of water which dissolves in the bromine can be neglected. He adds to the aqueous solution some substance which does not dissolve in bromine perceptibly, such as potassium bromide or sulphuric acid. The three phases, when in equilibrium, have still the same concentration of liquid bromine and of bromine vapor. Therefore the solubility of the bromine in the liquid cannot have changed. It does change experimentally; therefore, in order to reconcile the reasoning with the facts, he concludes that the apparent change, decrease or increase, is due to chemical action, and that the amount of bromine dissolved as such remains unchanged. This may be true in the special examples studied by Wildermann.[1] That I cannot say; but it is not true that it is a necessary theoretical conclusion, and there is no proof that it is correct in any case. If, instead of adding potassium bromide, we add to the water some liquid in which bromine is readily soluble, the amount of bromine dissolved will increase without there being any reason to assume chemical action in order to account for it. Bromine is not a good substance to consider, because there are so few liquids soluble in water in which it dissolves without decomposition, and also because we cannot ignore the solubility of the added substance in it. Let us rather treat the case when we have iodine instead of bromine. Suppose we have the system, solid iodine, a solution of iodine in water, and vapor of iodine and water; we add alcohol to the solution. The concentrations of the solid iodine and the iodine vapor will remain practically unchanged; therefore the solubility of iodine in the water and alcohol should remain unchanged according to Wildermann. As a matter of fact it does change, and I do not see how this variation can be attributed to chemical action unless all solution is defined as chemical action, which begs the question, though very possibly true. There may be a radical difference between the action of the alcohol and the action of potassium iodide; but that difference has not been shown. As far as I can see, Wildermann's conclusions require that adding alcohol to a saturated salt solution should have no effect on the concentration of the salt, because the equi-

[1] See Jakovkin, Zeitschr. f. ph. Chem., XIII. 539. 1894.

librium between the solid salt and its own vapor would remain unchanged.

Early in this paper I proposed the word "solute" as something distinct from "solvent," and it is necessary for me to justify that distinction. The usual way of looking at binary solutions is to consider them as mixtures, and that it is purely arbitrary which of the two substances we consider as solvent and which as dissolved substance. The following citations will show what the prevailing opinion at the present moment is.

Lothar Meyer, after pointing out that in alcohol-water mixtures it depends on the nature of the semipermeable membrane which substance exerts the osmotic pressure, says:[1] "Mit der Beschaffenheit der Membran tauschen beide Stoffe die Rollen; es ist daher eine Willkür wenn wir den einen als gelöst, den anderen als das Lösungsmittel bezeichnen." Ostwald is consistent to the bitter end, saying:[2] "Lösungsmittel ist derjenige Stoff des Gemenges, welcher bei dem betrachteten Vorgange ausgeschieden wird." This view is heroically logical, for it means that when a salt crystallizes from a saturated solution, the mother liquor consists of water dissolved in the salt.

Nernst's position on the subject is doubtful. He puts solutions under the head of physical mixtures and remarks:[3] "Die verdünnten Lösungen sind Gemische welche eine Komponente in grossem Ueberschuss zu den übrigen enthalten; erstere bezeichnen wir in diesem Falle als das Lösungsmittel, letztere als gelöste Stoffe." On the other hand, he draws a distinction between freezing out the solvent and crystallizing out the solute.[4] He does not accept the view that the salt is the solvent in a saturated solution; but he does not suggest in any way that there may be different laws for the solute and the solvent. Planck is very clear and precise; he defines dilute solutions in almost the same words as Nernst, and goes on:[5] "Bei einer beliebigen Lösung kann jeder Bestandtheil derselben als Lösungsmittel oder als gelöster Stoff aufgefasst werden." This means that in a mixture of two liquids

[1] Zeitschr. f. ph. Chem., V. 24. 1890.
[2] Ibid., XII. 394. 1893.
[3] Theoretische Chemie, p. 115.
[4] Ibid., p. 393.
[5] Grundriss der Thermochemie, p. 131.

either may be considered as the dissolved substance, and will therefore decrease the partial vapor pressure of the other, and this decrease of the vapor pressure will be greater the greater the concentration of the dissolved substance. This is not in agreement with the facts. A saturated solution of ether in water has the same partial vapor pressures as a solution of water in ether saturated at the same temperature.[1] For the moment we will consider ether as the dissolved substance. In the first solution, the volume concentration is roughly 10 per cent; in the second, about 99 per cent at 20°; and yet this enormous change of concentration has no effect on the partial vapor pressures. The figures are still more remarkable if we consider solutions of chloroform in water and water in chloroform, when one of the components is present in infinitesimal quantities. We must assume one of two things: either that our present formula for the change of the vapor pressure with the concentration is all wrong, since it does not admit of the vapor pressure of one of the components passing through a minimum; or that there is a difference between solvent and solute, and that each has its own law expressing the change of its vapor pressure with the concentration. This time I prefer the second assumption, with all that it implies. The equations of van 't Hoff and Raoult are the rough statements of the laws for the solvent. The corresponding expressions for the solute have not yet been worked out. The distinction between solvent and solute is very clear in solid solutions of metals in metals. Starting from either of two pure metals a depression of the freezing point is noted when the other is added, the two curves thus formed meeting at the melting point of the eutectic alloy. Here there can be no question that along one curve the first metal is solvent, while on the other it plays the role of solute. In the case of two partially miscible liquids there is also no difficulty in determining which is solvent and which solute. When ether and water are shaken together, the upper layer contains water as dissolved substance, the lower ether. With completely miscible liquids having a maximum (or minimum) vapor pressure at some concentration, such as propylalcohol and water (formic acid and water), it is probable

[1] Wied. Ann., XIV. 219, 1881; Ostwald, Lehrbuch, I. 644.

that the change of solvent occurs at the concentration corresponding to the maximum (or minimum) vapor pressure. With such things as ethylalcohol and water, which are infinitely miscible and which show no maximum or minimum vapor pressure, it is impossible at present to say at what concentration alcohol ceases to be the solvent and water assumes that duty. As soon as we have worked out the relation between the concentrations in the solution and in the vapor, I feel certain that we shall find that it requires two curves to express the relation, and not one. The intersection of these curves will be the point where the solvent changes. I look upon my own results with ternary mixtures as very significant in this respect, the change from one curve to another coming at the point where the precipitate or the solvent changed. It is interesting to note that at the point, for instance, where an excess of one of the partially miscible liquids first has no effect, the solubility curve of the dissolved substance has a "break." The possibility of such a case has always been denied except by the upholders of the "hydrate theory."

The effect of temperature on the various equilibria will form the subject of a special paper, and I shall reserve for it the discussion of changes of temperature coefficient at the intersections of two curves, one or two very striking instances of which I have come upon incidentally in my work so far. I hope also to be able to present a paper on equilibrium in two liquid layers, a subject which is of especial interest because the theoretical treatment based on the experimental work in this paper gives results which are not in accordance with the assumptions on which Nernst bases his Distribution Law. Besides, there is the application of the Mass Law to the case where one or more of the components is solid, and to the instances where there is an increase instead of a decrease of solubility.

The results of this paper may be summarized briefly as follows:

1. The equilibria between two partially miscible liquids and a consolute liquid follow the Mass Law.

2. There are four sets of equilibria corresponding to four different series of solutions.

3. If the two liquids are practically non-miscible, there are only two sets of equilibria.

4. The reacting weights of the liquids studied were not functions of the concentration, — possibly with one exception.

5. There is a fundamental difference between the solute and the solvent.

6. The solubility curve of a substance in a varying mixture of two liquids at constant temperature has a break.

TABLE XXIII.

$y = 3$ c.c. Propylbutyrate; x = c.c. Water; z = c.c. Alcohol.

Formula $x\,(y - 0.002\,x)^{0.378}/z^{1.378} = C$; $\log C = \bar{1}.651$.

	x.		
z.	Calc.	Found.	log C.
3	—	1.19	—
6	3.49	3.55	$\bar{1}$.658
9	6.11	6.13	$\bar{1}$.652
12	9.05	9.05	$\bar{1}$.651
15	12.31	12.31	$\bar{1}$.651
18	15.92	15.90	$\bar{1}$.650
21	19.68	19.68	$\bar{1}$.651
24	23.72	23.72	$\bar{1}$.651
27	27.92	27.84	$\bar{1}$.650
30	32.20	32.10	$\bar{1}$.649
33	36.71	36.71	$\bar{1}$.651
36	41.66	41.55	$\bar{1}$.650
39	46.64	46.49	$\bar{1}$.649
42	51.56	51.60	$\bar{1}$.652
45	56.80	56.90	$\bar{1}$.652
48	62.64	62.40	$\bar{1}$.649
51	67.84	68.00	$\bar{1}$.652
54	73.93	73.85	$\bar{1}$.650
			$\bar{1}$.651

TABLE XXIV.

$y = 3$ c.c. Ethylpropionate; x = c.c. Water; z = c.c. Alcohol.
Formula $x\,(y - 0.03\,x)^{0.39}/z^{1.39} = C$; $\log C = \bar{1}.931$.

	x.		
z.	Calc.	Found.	log C.
3	2.36	2.32	$\bar{1}$.924
6	6.89	6.87	$\bar{1}$.930
9	12.38	12.35	$\bar{1}$.930
12	19.10	19.17	$\bar{1}$.933
15	27.12	27.12	$\bar{1}$.931
18	36.84	36.84	$\bar{1}$.931
21	50.35	50.42	$\bar{1}$.932
24	—	∞	$\bar{1}$.930

TABLE XXV.

$y = 3$ c.c. Propylpropionate; x = c.c. Water; z = c.c. Alcohol.
Formula $x\,(y - 0.0065\,x)^{0.45}/z^{1.45} = C$; $\log C = \bar{1}.733$.

	x.		
z.	Calc.	Found.	log C.
3	—	1.58	—
6	4.45	4.70	$\bar{1}$.757
9	8.27	8.35	$\bar{1}$.738
12	12.25	12.54	$\bar{1}$.743
15	17.04	17.15	$\bar{1}$.736
18	22.27	22.27	$\bar{1}$.733
21	28.00	27.83	$\bar{1}$.731
24	34.20	33.75	$\bar{1}$.727
27	40.80	40.24	$\bar{1}$.727
30	47.95	47.15	$\bar{1}$.725
33	55.70	54.65	$\bar{1}$.725
36	63.50	63.18	$\bar{1}$.731
39	72.25	71.59	$\bar{1}$.729
42	81.15	83.05	$\bar{1}$.743
45	91.30	93.91	$\bar{1}$.746
48	102.00	107.46	$\bar{1}$.756
			$\bar{1}$.737

Table XXVI.

$y = 3$ c.c. Propylacetate; x = c.c. Water; z = c.c. Alcohol.
Formula $x\,(y - 0.03\,x)^{0.25}/z^{1.25} = C$; $\log C = 0.166$.

	x.		
z.	Calc.	Found.	log C.
3	4.44	4.50	0.170
6	10.57	10.48	0.163
9	17.75	17.80	0.167
12	25.95	26.00	0.167
15	35.72	35.63	0.165
18	46.50	47.50	0.178
21	59.00	58.71	0.164
24	—	∞	0.168

Table XXVII.

$y = 3$ c.c. Butylacetate; x = c.c. Water; z = c.c. Alcohol.
Formula $x\,(y - 0.007\,x)^{0.3}/z^{1.3} = C$; $\log C = \bar{1}.912$.

	x.		
z.	Calc.	Found.	log C.
3	—	2.08	—
6	6.06	6.08	$\bar{1}$.914
9	10.29	10.46	$\bar{1}$.920
12	15.04	15.37	$\bar{1}$.922
15	20.10	20.42	$\bar{1}$.918
18	25.64	25.60	$\bar{1}$.911
21	31.49	31.49	$\bar{1}$.912
24	37.60	37.48	$\bar{1}$.911
27	44.05	43.75	$\bar{1}$.909
30	50.74	50.74	$\bar{1}$.912
33	58.00	59.97	$\bar{1}$.927
			$\bar{1}$.916

TABLE XXVIII.

$y = 3$ c.c. Amylacetate ; x = c.c. Water ; z = c.c. Alcohol.

Formula $x\,(y - 0.02\,x)^{0.294}/z^{1.294} = C$; log $C = \bar{1}.861$.

	x.		
z.	Calc.	Found.	log C.
3	—	1.76	—
6	—	4.24	—
9	9.03	9.03	$\bar{1}$.861
12	13.11	13.24	$\bar{1}$.866
15	17.43	17.52	$\bar{1}$.864
18	22.22	22.22	$\bar{1}$.861
21	26.99	26.99	$\bar{1}$.861
24	32.24	32.14	$\bar{1}$.860
27	37.59	37.23	$\bar{1}$.856
30	42.78	42.66	$\bar{1}$.859
33	48.41	48.41	$\bar{1}$.861
			$\bar{1}$.861

TABLE XXIX.

$y = 3$ c.c. Propylformiate ; x = c.c. Water ; z = c.c. Alcohol.

Formula $x\,(y - 0.04\,x)^{0.38}/z^{1.38} = C$; log $C = \bar{1}.967$.

	x.		
z.	Calc.	Found.	log C.
3	2.82	2.83	$\bar{1}$.969
6	7.52	7.50	$\bar{1}$.966
9	13.65	13.50	$\bar{1}$.962
12	21.30	21.60	$\bar{1}$.973
15	30.95	30.60	$\bar{1}$.962
18	52.40	53.00	$\bar{1}$.972
21	—	∞	$\bar{1}$.967

TABLE XXX.

$y = 3$ c.c. Butylformiate ; x = c.c. Water ; z = c.c. Alcohol.

Formula $x\,(y - 0.01\,x)^{\frac{1}{2}}/z^{\frac{4}{3}} = C$; $\log C = 0.057$.

	x.		
z.	Calc.	Found.	log C.
3	3.43	3.45	0.060
6	8.71	8.83	0.063
9	15.02	14.75	0.049
12	22.32	21.45	0.041
15	30.25	29.65	0.048
18	39.00	39.00	0.057
21	48.80	51.80	0.083
24	—	∞	0.057

TABLE XXXI.

$y = 3$ c.c. Amylformiate ; x = c.c. Water ; z = c.c. Alcohol.

Formula $x(y - 0.005\,x)^{0.35}/z^{1.35} = C$; $\log C = \bar{1}.808$.

	x.		
z.	Calc.	Found.	log C.
3	—	1.80	—
6	4.92	5.17	$\bar{1}$.829
9	8.54	8.77	$\bar{1}$.820
12	12.63	12.64	$\bar{1}$.809
15	17.10	17.01	$\bar{1}$.806
18	21.90	21.86	$\bar{1}$.807
21	27.06	27.06	$\bar{1}$.808
24	32.50	32.31	$\bar{1}$.805
27	38.31	38.31	$\bar{1}$.808
30	44.40	44.50	$\bar{1}$.809
33	50.71	50.71	$\bar{1}$.808
36	57.20	57.82	$\bar{1}$.813
39	62.70	65.21	($\bar{1}$.830)
42	71.35	77.05	($\bar{1}$.842)
45	78.75	85.10	($\bar{1}$.842)
48	86.55	94.20	($\bar{1}$.845)
			$\bar{1}$.811

ON THE CHANGES IN LENGTH PRODUCED IN IRON WIRES BY MAGNETIZATION.

BY LOUIS TRENCHARD MORE.

THIS investigation was undertaken at the suggestion of Professor Rowland, and has for its object the finding of a relation between the change of length produced in iron wires by magnetization, and the intensity of magnetization existing in the wire. It was hoped thus to obtain results that would be comparable, and to avoid certain errors common to all previous work.

Historical.

That magnetizing an iron rod causes it to alter in length was first discussed by Joule[1] in 1847. His attention was called to the phenomenon by a machinist of Manchester, who imagined that the volume of a mass of iron was increased by magnetizing it. Joule, to test the opinion of the machinist, immersed a mass of iron in a closed vessel full of water in which stood a fine capillary tube. When the iron was strongly magnetized, the height of the column of water in the tube remained unaltered, showing that within the limits of accuracy of his apparatus, for the intensity employed, the volume of the iron was unchanged. Bidwell[2] has also investigated this subject and found, on the contrary, that the volume was altered by magnetization. The volume diminishes at first and attains a minimum; it then increases until with sufficiently intense fields the original size is regained; after reaching this point the volume continues to increase. As a consequence of this relation, if Joule had used an intensity either greater or less, he probably would have noticed a change in the volume. Joule afterwards, by means of a system of levers, found that the length

[1] Joule, Phil. Mag. (3), Vol. XXX., pp. 76, 225.
[2] Bidwell, Proc. Roy. Soc., Vol. LVI., p. 94.

of a rod was increased by the magnetizing force, and gave as a result of his observations the following laws: —

1. When soft iron rods are magnetized, their length is increased and the elongation is approximately proportional to the square of the magnetizing force.

2. Tension applied to the rod diminishes the elongating effect, and — "In the case of a bar one foot long and one-quarter inch in diameter, a tensile force of about 600 pounds caused all the phenomena of changes of length to disappear."

3. "That the elongation is greater, for the same intensity of magnetism, in proportion to the softness of the metal. It is greatest of all in the well annealed iron bars, and least in hardened steel. This circumstance appears to me to favor the hypothesis that the phenomena are produced by the attractions taking place between the magnetized particles of the bar, an hypothesis in perfect accordance with the law which I have pointed out," — that the elongation was proportional to the square of the intensity of magnetization.

The first two laws pointed out by Joule have been often confirmed, but the third seems to rest on a single experiment, and until very recently there have been no published records, that I have seen, bearing on the result found by Joule. Shelford Bidwell[1] while investigating the subject obtained results the converse of Joule's, — that not only hardening, but also annealing, the iron diminished the elongating effect. He mentions one specimen that when annealed contracted in length instead of elongating upon the application of the feeblest magnetizing force.

Barrett[2] in 1870 discovered that nickel when magnetized contracts instead of elongating. Three years later A. M. Mayer[3] published an account of his experiments on this subject. His results in the main verified Joule's observations, with the exception of the action of hard steel. This discrepancy was shown some years later by Bidwell to follow from their different methods of experimenting. Joule applied a current of the same intensity but once, and both on making and on breaking the circuit observed an elongation; while Mayer used specimens already perma-

[1] Bidwell, Proc. Roy. Soc. Vol. LV., p. 228. [2] Barrett, Nature, 1882.
[3] Mayer, Phil. Mag. Vol. XLVI., p. 179.

nently magnetized, and on observing the temporary magnetization, found a contraction on making circuit and an elongation on breaking it.

Mayer also observed hysteretic effects; that is, the elongation due to a magnetizing force was less if the force had been reached by successively increasing values, than it was if the current had been decreased from a maximum, the rod remaining slightly elongated after the magnetizing force had been removed, an effect analogous to the lagging of the induction behind the magnetizing force. Nagaoka[1] has discussed this phenomenon in the article cited, and has obtained complicated curves showing the complete cycle of the hysteretic phenomena for both iron and nickel.

The experiments mentioned were limited to comparatively weak fields; the work of finding the effects due to intense fields has been most thoroughly done by Shelford Bidwell,[2] who has found that rods do not continue to elongate indefinitely with increasing strengths of field, as the other investigators supposed, but that a maximum value is after a time reached. The rod then begins to shorten, and very intense fields produce an absolute contraction which approaches a limiting value asymptotically. He also experimented with rings of iron, and with rods of steel, nickel, manganese steel, cobalt, and bismuth.

Investigations upon this subject have also been made by Berget,[3] Nagaoka,[4] Lochner,[5] Jones,[6] and Bock.[7]

For convenience, I have collected in a summary the results obtained by the different observers.

Soft Iron. — Soft iron elongates when magnetized. The elongation attains a maximum and then diminishes with increasing strength of field until a state is reached when the rod returns to its original length. Further increase of field causes the rod to contract.

[1] Nagaoka, Phil. Mag., Vol. XXXVII., p. 131.

[2] Bidwell, Proc. Roy. Soc., Vol. XXXVIII., p. 265; Vol. XL., pp. 109, 237; Vol. XLVII., p. 469; Vol. LV., p. 228; Trans. Roy. Soc., Vol. CLXXIX. (A), p. 205.

[3] Berget, Comp. Rend., tom. CXV., p. 722.

[4] Nagaoka, Wied. Ann., LIII., pp. 481, 487; 1894.

[5] Lochner, Phil. Mag., Vol. XXXVI., p. 504; 1893.

[6] Jones, Phil. Mag., Vol. XXXIX., p. 254; 1895.

[7] Bock, Wied. Ann.; 1895.

There is no minimum length, the rod approaching asymptotically a limiting value.

With a given strength of field, both hardening and annealing diminish the elongation and increase the contraction shown by the rod before it was subjected to these operations. (Bidwell.)

Tension also diminishes the elongation and increases the contraction of the rod.

For a sufficiently great tension no elongation occurs, the rod contracting upon the application of the smallest magnetizing forces.

For a given length, the effects both of elongation and of contraction are greater for thin than for thick rods. (Bidwell.)

S. J. Lochner[1] comes to the conclusion from his own experiments that the converse is true, — that thick bars show greater elongation than thin ones.

Very little reliance can be put in these last experiments, and the dependence of the change of length upon the ratio of the length to the diameter cannot be inferred from them. Bidwell used three rods 10 cm. long and 2.65, 3.65, and 6.25 mm. in diameter, and assumed them to be of iron of similar composition. It is well known that different specimens of iron, apparently similar in structure, give results that vary 25 per cent and more, so that the small variations in the change of length noted by him cannot safely be said to be due to the differences in their diameters, especially as he made no determination of the permeability. Lochner avoided this error by testing an iron rod, and then after having cut off a portion, testing it again. He, however, took no precaution to have the field uniform. His solenoid was nearly four times as long as the shortest rod used, and the ratio of the diameter to the length of the rod was only 1 to 32. Besides the uncertainties introduced by such a poor arrangement for a uniform field, great errors would be produced by the strong poles created at the ends of the rod.

Object of Experiment.

A careful study of the results obtained for iron by the different observers will show that although the general appearance of their

[1] Lochner, *loc. cit.*

curves is very similar, yet their absolute values vary widely, two specimens often having maximum elongations that differ 20 or 30 per cent. There is, unfortunately, no way of comparing these results, for it has been the custom to use as co-ordinates the change of length and the intensity of the external field. For a given apparatus this intensity depends only on the current used, and not at all on the specimen to be examined. The elongation is dependent upon the intensity of magnetization in the wire, and this is the quantity that varies with the specimen employed. For that reason the relation should be found between these two quantities.

It is, of course, essential to have the rod uniformly magnetized throughout its length; that is, the field should be uniform and there should be no free poles. These conditions may be best obtained in one of two ways, either by having the metal in the shape of a ring, and observing the change in diameter of the ring when magnetized by a solenoid wound upon it, or by using long wires of the metal. In the second case only the middle part of the wire should be observed, and the solenoid used to magnetize it should be considerably longer than the portion of the wire experimented upon. In this investigation the latter method was chosen, as rings are less convenient, and also because it was desirable to observe the effects of tension in the metal.

When a rod of iron is magnetized, the change in length observed is due to several causes, — three at least, — and to obtain a correct idea of the phenomena these causes and their effects should be separated. There is first the *direct action* of the magnetism, and this may possibly be due to the orienting of the magnetized particles of the rod. Secondly, there are indirect actions of the magnetism which tend to change the length of the rod. These indirect actions are the mechanical stresses created in the rod by the magnetism. The first of these mechanical stresses is the tractive force of the magnet and is measured by $\frac{B^2}{8\pi}$. That this force exists, tending always to contract the rod, is seen from the fact that if the magnet is cut in two, the ends are held together by a force $\frac{B^2}{8\pi}$ per square centimeter, showing that this force

must always be present when a rod is magnetized. The agreement between the theoretical value $\frac{B^2}{8\pi}$ and the experimental law for the lifting power has been recently shown by E. Taylor Jones.[1] This effect for high intensities of magnetization is a large one, and becomes one of the most important factors in the observed changes in length. The second of these mechanical stresses is the effect due to the change in Young's modulus when the rod is magnetized. That the elasticity is influenced by magnetization was shown theoretically by J. J. Thomson,[2] and the phenomenon was observed last year in some experiments made by the writer; but no quantitative results could be obtained beyond the fact that the elasticity for soft iron was slightly diminished. A. Bock[3] concludes from his work that the decrease in elasticity must be less than one-half per cent. If the wire is not stretched by weights, this decrease in the elasticity will affect only the contraction due to the $\frac{B^2}{8\pi}$ force. On the other hand, if the wire is loaded with weights, this effect becomes very marked, since a large quantity, the stretch of the wire by the great weight, is altered. This question will be more fully discussed later in the paper.

If these two indirect actions were allowed for, there would remain only the direct action of the magnetism upon the metal under a constant tension. This latter relation would evidently furnish comparable results, and may in the future throw some light upon the action of magnetism on matter.

Apparatus.

Since it was necessary to obtain the modulus of elasticity for the specimen experimented on, in order to make a proper correction for the electro-magnetic stress, and as I wished to observe the effects of mechanical stress, it was convenient to experiment on thin, long wires of the metal placed in a vertical position. To magnify the phenomena a system of levers was used, involving the

[1] Jones, *loc. cit.*

[2] J. J. Thomson, Application of Dynamics to Physics and Chemistry, p. 58.

[3] Bock, *loc. cit.*

tilting of a mirror mounted on three legs very close together, — a method first invented by Professor Rowland. The general plan of using levers and a jacket cylinder was suggested to me by Dr. Ames.

The wire to be tested was suspended from a tall tripod standing on a stone slab that rested on two brick piers. A hole was cut in the slab so that the wire could be passed through it and the free end permitted to almost reach the floor of the room. A hollow brass cylinder 1.6 cm. in diameter and about 68.0 cm. long (Fig. 1) was screwed to the wire at a point *a*, a short distance above the stone slab; a loosely fitting cork plug in its open upper end served to keep the wire in the axis of this cylinder. To the cylinder was screwed the brass arm *deb*. At *e* was screwed a hard steel support for the fulcrum of the lever. The side view of the support was of this shape, |_____|, the two projections supporting the knife edge, and the body of the lever passing between them. To keep the lever horizontal and to register changes of length, another knife edge with its blade upwards was embedded in the lever. This knife edge pressed against the piece *mn*, which was screwed to the wire at *m*. Now if the points *m* and *a* were separated or brought close together, the knife edges would no longer remain in a horizontal line, and the lever would tilt. The end *p* of the lever, and the surface *d* of the arm *bed*, were made planes to support a small brass table having on its upper face a vertical plane mirror, and for legs three short bits of needles, two of which stood on *p* and one on *d*. By this arrangement the change in the length of *ma*, already magnified by the lever, was much more apparent when the tilting of the mirror table was read by means of a telescope and scale. The most important dimensions are: —

Length of wire stretched, *ma*	69.9 cm.
Ratio of lever arms	$\frac{119}{4.65}$
Distance between needle points . . .	2.3 mm.
Distance to telescope and scale . . .	1660.0 mm.

The multiplying power of the apparatus was, therefore, —

$$\frac{119}{4.65} \times \frac{1660}{2.3} \times 2 = 36941.$$

And as the scale was graduated to millimeters, one scale division represented an actual change of length of 2.7×10^{-5} mm.

The coil to magnetize the wire stood on the stone slab and was long enough to reach just below the arm *ce*, so that the part of the wire *ma* experimented on was in a practically uniform field.

The principal dimensions of this coil were as follows: —

Length of coil	83.7	cm.
External diameter	6.3	cm.
Internal diameter	3.6	cm.
Number of wire	18.	
Number of turns	3045.	
Number of layers	7.	
Number of turns per cm.	36.4	
Resistance of coil	10.53	ohms
Strength of field per ampère	45.75	C.G.S.
Maximum field	260.00	C.G.S.

The core of this coil was made of two co-axal brass cylinders fastened together by the end plates of the coil. The space between these two cylinders was filled with water, which proved to be an excellent way of retarding the heat effects produced by the current.

Method.

I at first intended to measure the elongation and the corresponding induction simultaneously, but found this difficult, and so adopted a more convenient and apparently as accurate a method. This was to first measure the current used to produce a given intensity of magnetism and the consequent changes in length, and afterwards to get the relation between the strength of field and the intensity of magnetization in the iron, the apparatus in the meanwhile remaining untouched. As the laboratory is situated in the city, the work had to be done late at night after traffic had stopped; for, in spite of all the precautions that could be thought of, the shaking of the apparatus could not be prevented. For this reason the induction was not determined immediately after obtaining the elongation curve, but was done the next morning. The

induction was measured by the method of reversals.[1] The induction coil used for this purpose consisted of 200 turns of No. 36 wire wound in one layer on a paper cylinder slightly greater in diameter than the specimen. This little cylinder was slipped over the wire and fixed half-way between *am*. The galvanometer was calibrated by means of a standard coil having a wooden core. The secondary of the standard coil also consisted of 200 turns of wire, which plan saved much computation. The intensity was then calculated by the formula $B = H + 4\pi I$. Knowing the change in length and the intensity of magnetization for any current, of course the relation between these two quantities could be easily platted. The current was supplied by a battery of storage cells, and the resistance was regulated by a slide resistance of copper sulphate.

Results.

My first results were obtained from a specimen of moderately soft commercial iron wire, one millimeter in diameter. This wire was free from stress, except that due to the jacket cylinder screwed to it, which weighed 350 grams. The elongations given (Table I.) are those due to temporary magnetism, and each value is the mean of two or three readings which did not vary by more than one per cent. The current was applied suddenly, and as soon as a reading was taken the contact was broken. This operation was then repeated, the current being increased each time, and readings taken until the maximum current was reached. By this means temperature effects, which are relatively slow to act, were avoided.

Figure 3, *C* shows the relation between the elongation per unit length, $\frac{\delta l}{l} \times 10^7$, and the intensity of the field, *H*. This curve is seen to closely resemble in form those given by Shelford Bidwell. When the relation is expressed between change of length and intensity of magnetization, the curve takes the very different form given by Fig. 3, *B*. The wire slowly increases in length until an intensity of about 800 is reached. From that point, until a maximum length is attained, at an intensity of 1200, the elongation is

[1] Ewing, Lond. Electrician, April, 1894.

more marked. After reaching this maximum it rapidly contracts, the last portion of the curve being approximately a nearly vertical straight line. The figure also shows that the point of maximum length does not correspond to the point of greatest permeability of iron, as is seen by comparing this curve with the curve for permeability plotted on the same figure.

It is now necessary to make corrections for the contraction due to the $\frac{B^2}{8\pi}$ force. This contraction is obtained from the formula

$$\frac{\delta l}{l} \times 10^7 = \frac{B^2/8\pi}{M},$$

where M is the modulus of elasticity. My apparatus was well fitted for measuring the latter quantity. A weight of about five kilograms was hung on the wire, and the elongations due to the additions of weights of 20, 50, and 100 grams were read. From these Young's modulus was calculated in the usual manner. It was found to be 2.2×10^{12} for the specimen used. The contractions due to the $\frac{B^2}{8\pi}$ force were then calculated, and are given in column 6 of Table I.

The last column of this table was found by taking the difference between the observed elongation and the $\frac{B^2}{8\pi}$ contraction. The relation between this corrected elongation, due to what has been called the direct action of the magnetism, and the intensity of magnetization, is graphically shown by Fig. 3, *A*. The effect of this correction is to make the elongation much greater for a given intensity. The maximum value of the elongation is more than twice as great as the observed maximum. And the greatest intensity employed, 1300 C.G.S. units, produces an elongation and not a contraction as observed.

No correction for the change in elasticity was made. For the present case it would be insignificant. See discussion of errors at end of this paper.

TABLE I.

H	μ	I	B	$\frac{\delta l}{l}$ (obs.)	$\frac{\delta l}{l}\left(\frac{B^2}{8\pi}\right)$	$\frac{\delta l}{l}$ (corr.)
4.6	384.8	141	1770	+0.10	−0.7	+0.80
7.3	507.0	294	3700	—	—	—
10.5	752.4	623	7900	1.16	11.3	12.46
11.8	733.0	700	8800	—	—	—
12.8	726.7	739	9300	2.71	15.65	18.36
15.5	695.0	856	10770	5.71	20.98	26.69
19.7	627.0	981	12350	14.71	27.60	42.31
24.2	541.4	1041	13100	18.78	31.05	49.83
28.8	482.7	1104	13900	22.92	34.95	57.87
37.6	389.1	1163	14630	25.55	38.53	64.13
53.0	286.3	1203	15170	24.78	41.69	66.47
60.4	253.7	1214	15320	23.23	42.47	65.70
67.7	227.7	1222	15410	21.30	42.96	64.26
80.0	193.8	1228	15500	18.74	43.46	62.20
93.3	167.2	1235	15600	15.49	44.02	59.51
103.8	150.8	1238	15650	12.81	44.30	57.11
111.6	140.7	1241	15700	11.03	44.59	55.62
121.2	130.0	1244	15750	9.20	44.88	54.08
145.4	109.6	1257	15930	5.42	45.90	51.32
159.1	100.6	1261	16010	+1.94	46.37	48.31
186.5	86.9	1274	16200	−2.94	47.47	44.53
210.3	77.8	1281	16360	6.97	48.42	41.45
246.9	67.1	1299	16570	12.97	49.67	36.70
260.6	63.9	1304	16650	16.65	50.15	33.30

The effect of hardening the wire was next considered. A piece of the same quality of iron was heated to a bright red by passing a current through it, and then suddenly cooled. After the operation the wire was much harder, and only slightly burnt on the surface. The observed changes in length, those due to $\frac{B^2}{8\pi}$ and the corrected values are given in Table II. Hardening the iron makes the elongation smaller, and for the present specimen the least intensity of magnetization caused the iron to contract (Fig. 4, B). The corrected relation between the change of length and the intensity is shown by B'. On this plate is also plotted curve A, the same relation, taken from Fig. 3, for the wire when not hardened; and curve A' shows the corrected relations for the same.

Although the observed change for the hardened wire was a contraction, the contraction due to the $\frac{B^2}{8\pi}$ force was sufficient to bring the values above the zero axis. So, again, the direct action of the magnetization is to elongate the wire, and the corrected curve is very similar to the curve for the iron in its original state. The absolute values are, however, much diminished, and the maximum occurs, for this specimen, with a less intensity. The correction for the change of elasticity can be neglected as in the first case.

TABLE II.

H	I	B	$\frac{\delta l}{l}$ (obs.)	$\frac{\delta l}{l}\left(\frac{B^2}{8\pi}\right)$	$\frac{\delta l}{l}$ (corr.)
5.0	8	100	−0.00	−0.00	+0.00
8.7	53	670	0.05	0.08	0.03
12.8	272	3420	0.10	2.12	2.02
18.3	709	8920	0.20	14.41	14.21
27.4	923	11620	0.50	24.44	23.94
36.6	947	11940	0.70	25.77	25.07
45.7	1012	12770	1.85	29.48	27.63
57.2	1044	13180	3.10	31.42	28.32
68.5	1060	13390	4.10	32.42	28.32
96.5	1074	13600	7.40	33.43	26.03
118.8	1064	13490	5.44	32.93	27.53
138.5	1100	13560(?)	15.00	34.24	20.24
180.1	1126	14320	24.20	37.11	12.91

To find the effect of strain the wire was loaded with a weight of 750 kg. per square centimeter, and the relation between the elongation and the intensity of magnetization obtained as in the previous cases. The wire was then loaded with a weight of 1750 kg. per square centimeter, and the same relations found. The values are given in Tables III. and IV. The effects of strains are also shown graphically by Fig. 5, for the observed and corrected values. By consulting the figure it is seen that straining the wire produces the same change that hardening it does. The values are reduced, and the maximum points occur with less intense magnetization. The curves in this figure, as well as in the two former figures, marked no load, have an inaccuracy due to the

strain caused by the weight of the jacket cylinder, and should have ordinates a trifle greater.

Although the change in elasticity has hitherto produced little effect, as soon as the wire is much strained this correction becomes very important, and will modify the curves materially. This alteration in the curves it has not been possible to make, as the relation between the change in elasticity and the intensity of magnetization is unknown. However, from Bock's results some approximations may be arrived at; and these will be considered when the errors likely to occur in this investigation are discussed.

TABLE III.

H	I	B	$\frac{\delta l}{l}$ (obs.)	$\frac{\delta l}{l}\left(\frac{B^2}{8\pi}\right)$	$\frac{\delta l}{l}$ (corr.)
6.9	636	8000	+0.12	−11.58	+11.70
11.4	918	11550	0.62	24.14	24.76
18.5	1080	13600	2.63	33.46	36.09
24.2	1109	13960	3.73	35.27	39.00
30.2	1139	14340	3.87	37.19	41.06
34·7	1150	14480	3.48	37.94	41.42
43.9	1167	14700	2.42	39.09	41.51
55.8	1184	14930	+1.20	40.33	41.53
73.2	1204	15200	−0.39	41.79	41.40
81.8	1212	15300	1.16	42.35	41.19
100.6	1223	15470	4.41	43.30	38.89
122.5	1235	15640	9.10	44.24	35.14
148.1	1247	15820	14.13	45.27	31.14
157.7	1253	15910	15.80	45.80	30.00
175.1	1265	16070	18.58	46.71	28.13
192.1	1276	16230	25.05	47.65	22.60
228.6	1287	16400	29.03	48.65	19.62
251.4	1293	16500	32.13	49.26	17.13

Possible Errors.

It has been shown that the change in elasticity occurring when an iron wire is magnetized modifies the relation between the intensity of magnetization and the elongation; and it has been assumed that, except for heavily loaded wires, this effect will be

very small. Bock[1] found as a result of his experiments that magnetizing soft iron made it more incompressible, and that the change in elasticity was less than one-half per cent. He could not find any relation between the intensity of magnetization and change

TABLE IV.

H	I	B	$\frac{\delta l}{l}$ (obs.)	$\frac{\delta l}{l}\left(\frac{B^2}{8\pi}\right)$	$\frac{\delta l}{l}$ (corr.)
5.5	581	7300	−0.19	−9.64	+9.54
9.1	922	11600	0.58	24.35	23.77
13.7	1011	12720	1.26	29.27	28.01
21.5	1070	13470	2.90	32.83	29.93
27.4	1100	13860	4.06	34.76	30.70
36.6	1134	14280	5.90	36.89	30.99
45.7	1155	14560	7.36	38.36	31.00
54.9	1167	14710	9.68	39.14	29.46
73.2	1178	14880	13.84	40.05	26.21
86.9	1186	14990	16.64	40.65	24.01
96.0	1190	15050	19.74	40.97	21.23
111.1	1198	15160	23.63	41.58	17.95
128.0	1207	15290	27.87	42.29	14.42
139.4	1212	15370	32.51	42.74	10.23
150.9	1217	15440	33.97	43.12	9.15

in elasticity, so that it is impossible to give more than a probable maximum to the correction it is necessary to make in the curves given in this investigation.

If we take $B = 20{,}000$,

$$\frac{B^2}{8\pi} = \frac{1}{2\pi} \times 10^8 \text{ dynes per square centimeter,}$$

and

$$\frac{\delta l}{l} \times 10^7 = -\frac{\frac{1}{2\pi} \times 10^8}{M} = -73.$$

A change of one-half per cent in $\frac{\delta l}{l}$ would give a correction of $0.005 \times 73 = .4$ of a unit. This correction may therefore be neglected, as errors greater than this would occur in measuring the induction.

[1] Bock, *loc. cit.*

It was also assumed that when the wire was not stretched, except by the jacket, the correction might be neglected.

The weight of this jacket was 350 grams, or 42000 dynes per square centimeter; then

$$\frac{\delta l}{l} \times 10^7 = \frac{42000}{M} \times 10^7 = 200.$$

A change of one-half per cent in 200 is 1.0. Consequently the curves marked no load are not significantly changed by making this correction.

But when we consider the case of the wire loaded with 750 kg. per square centimeter, we get, by a similar calculation, a correction of 15 units; and for the wire under the greatest tension, 1750 kg. per square centimeter, a correction of 40 units. Since the magnetism makes the metal more incompressible, this correction enters as a contraction, and must be added to the ordinates of the curves plotted on Fig. 5. It is impossible to say how these curves would be affected by the correction, but probably the correction increases with the intensity of magnetization.

Changes in the length of the wire due to variations in temperature are another possible source of error. These changes take place more slowly than the elongation produced by the magnetization, so that the two effects may be separated. To retard them still more, the core of the solenoid was made of two co-axal cylinders, and the space between them filled with water. With this arrangement the current could be applied, the reading taken, and the current shut off, before the gradual change of the zero point due to the heating of the wire by the current could be observed. Changes in the temperature of the room when they did occur were too slow to noticeably affect the results.

Summary.

The following is a summary of the results obtained by this investigation: —

1. A relation has been obtained between the elongation due to magnetization and the intensity of magnetization for soft iron. When the elongation has been corrected for the contraction caused

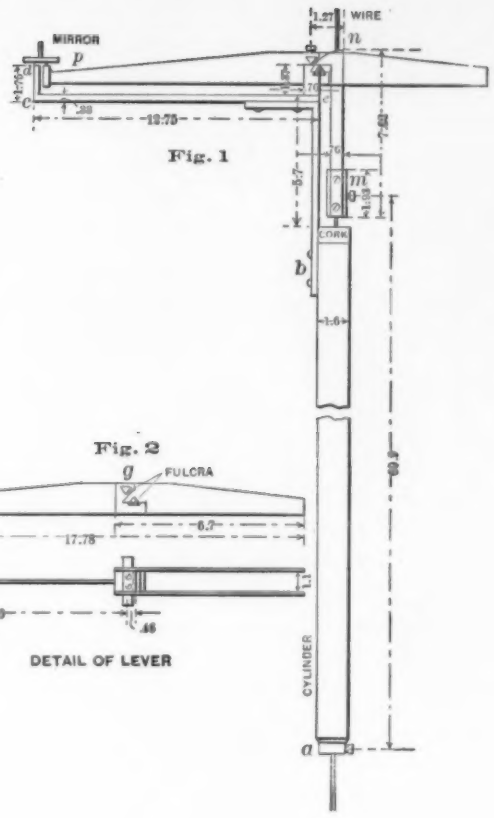
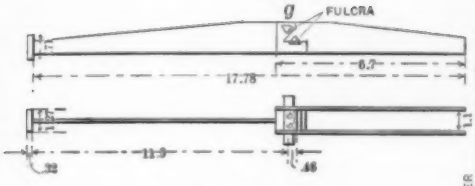
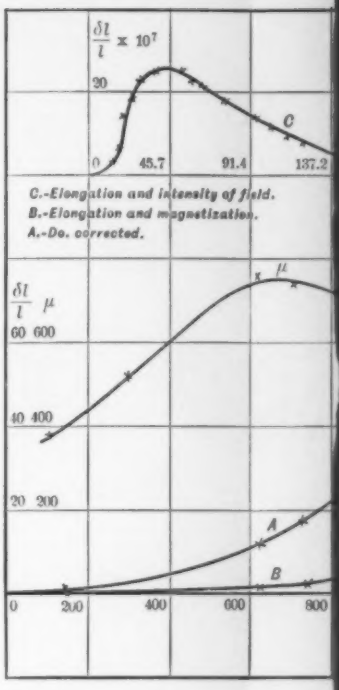


Fig. 1



DETAIL OF LEVER

Fig. 2



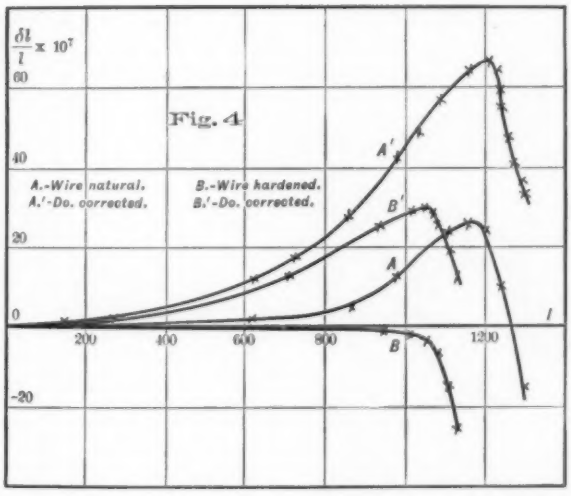


Fig. 4

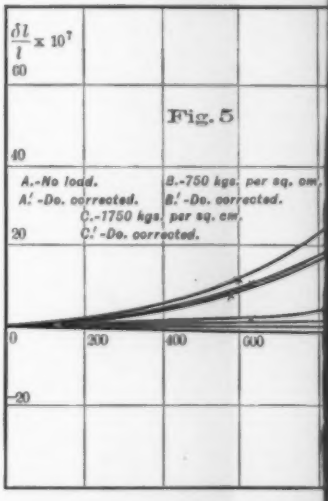


Fig. 5

MORE: CHANGES IN LENGTH DUE TO MAGNETIZATION.

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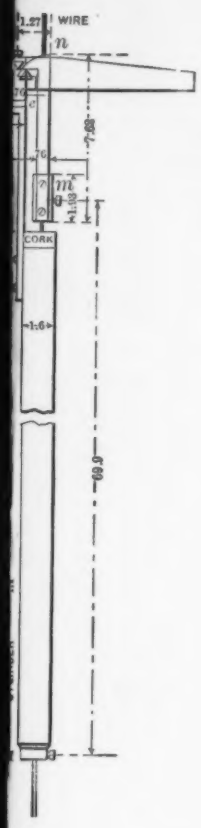
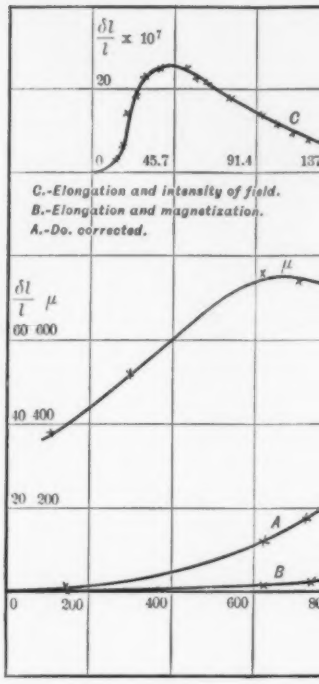




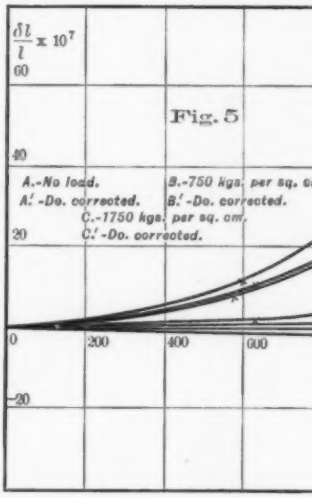
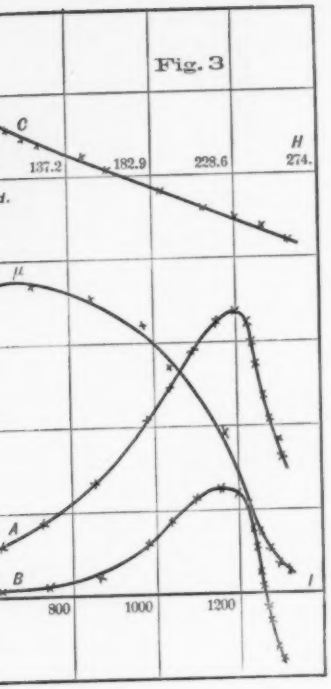
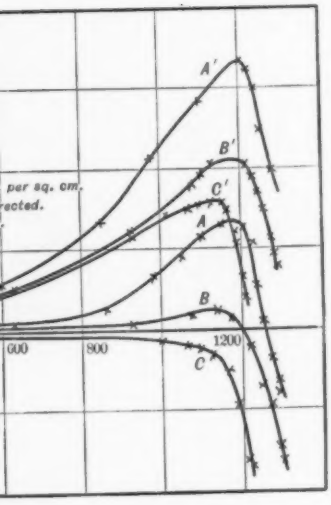


Fig. 5

ANGES IN LENGTH DUE TO MAGNETIZATION.

Fig. 3


C
137.2
182.9
228.6
H
274.
A
B
800
1000
1200


A'
B'
C'
A
B
C
per sq. cm.
rected.
600
800
1200

by the $\frac{B^2}{8\pi}$ force, the relation may be expressed thus: A rod of iron elongates slowly until an intensity of about 800 is reached. After that point the rod elongates more rapidly and attains a maximum value of about 60×10^{-7} part of its length at an intensity of 1200. With greater intensities the elongation diminishes, the curve being approximately a nearly vertical straight line.

2. Hardening the wire diminishes the corrected elongation, and the wire attains its maximum length in a less intense field.

3. It has been shown that when the wire is stretched, the change in elasticity due to the magnetization produces an important effect. Neglecting this correction, the effects of stretching are similar to those caused by hardening the wire.

Before concluding I would acknowledge my indebtedness to Professor Rowland, not only for suggesting the investigation to me, but also for his assistance and kind consideration afterwards.

I also owe many thanks to Dr. Ames and to Dr. Duncan for advice and help.

NOTES.

Eli W. Blake. — Professor Blake was born in New Haven, Conn., April 20, 1836; he died at Hampton, Conn., October 1, 1895.

He was graduated at Yale in the class of 1857. After graduation he devoted a year to teaching in a private school at Unionville, Conn., and subsequently another year to study in the Sheffield Scientific School. Next he studied in Germany for three years and a half, — at Heidelberg under Bunsen and Kirchhoff, at Marburg under Kolbe, at Berlin under Dove and Magnus. He devoted his attention to both chemistry and physics; for although he became ultimately a physicist, it was his original intention to be a chemist. Upon his return, he was for a year (1866–67) professor of chemistry and physics in the University of Vermont and State Agricultural College; for the year 1868–69, acting-professor of physics in Columbia College, New York; and later, professor of physics and mechanic arts at Cornell University, then just opened. From 1870 to 1895, he was Hazard professor of physics in Brown University. Owing to the ill health of a member of his family, he resigned this chair to take effect June, 1895; but before that date he was taken slightly ill himself, and he did not recover. His constitution was weakened by the continuous and confining labors incidental to his profession, and he died after an illness of about five months. He was twice married, his second wife surviving him. By his first wife, who was a sister of Professor Ogden N. Rood, he leaves two children, — a son, Eli Whitney Blake, and a daughter, Mrs. Barclay Hazard, of California.

Professor Blake received the honorary degree of A.M. from the University of Vermont and State Agricultural School, and the degree of LL.D. from Brown University.

While he was at Brown University, that institution received from the late George F. Wilson, a bequest of above $100,000, to be devoted to a physical laboratory. Wilson Hall, immediately erected, will long stand, a monument to the generous donor; but it is also a monument to Professor Blake, to whose able, unwearied, and conscientious labors are largely due its appropriate and convenient arrangements.

Professor Blake had mental and spiritual qualities of a very high order. His sincere devotion to truth led to a conscientiousness, a high sense of honor, and great moral courage, — beautiful elements of character which he

wore like parts of himself, and in no sense as garments, to be put on and off. His interest in his students was intense and genuine. He was one of the most unselfish of men. He was the warmest and truest of friends. While he devoted little time to society he was welcomed as a brilliant writer and talker, and as an affectionate and delightful companion.

By nature, Professor Blake had very strong mechanical and scientific tendencies. Indeed, they were in the blood; for he was grand-nephew of Eli Whitney, whose cotton-gin was an epoch-making machine. He was son of Eli Whitney Blake, of New Haven, himself a scientific man, manufacturer, and an inventor.

Professor Blake was an indefatigable worker in the laboratory. He was there from morning to night. He pursued experimental study in every department of physics, oftenest with apparatus of his own designing and the product of his own hands. Of his published work, the best known is the *Method of recording Articulate Vibrations by Means of Photography* (American Journal of Science, vol. 16, 1878).

JOHN HOWARD APPLETON.

The American Association for the Advancement of Science. — The forty-fourth meeting of the association was held at Springfield, Massachusetts, beginning on August 28, 1895. The hall for the general sessions, and the rooms for the various sections and affiliated societies, were conveniently located in the Christian Association Building and the Springfield High School, while a hall in the immediate neighborhood was put at the disposal of the association for the presentation of such papers as required the use of the lantern. Among the many pleasant features of the meeting were the receptions offered by the citizens of Springfield to the members of the association, and the Saturday excursion to Amherst and Northampton.

Although the attendance of physicists was not above the average, the sustained interest shown in the sessions of Section B, as well as the number and character of the papers presented, justify the remark made by several members that the meeting of this section at Springfield was one of the most satisfactory in recent years. In addition to the address of the vice-president, Professor W. Le Conte Stevens, on "Recent Progress in Optics," twenty-five papers were read as follows: —

"Expansion of Jessop's Steel, measured by Interferential Method," by E. W. Morley and William A. Rogers; "Flow of Alternating Current in an Electrical Cable," by M. I. Pupin; "The Most General Relation between Electric and Magnetic Force and their Displacements," by M. I. Pupin; "Relations of the Weather Bureau to the Science and Industry of the Country," by Willis L. Moore; "Solar Magnetic Radiation and Weather Forecasts," by Frank H. Bigelow; "Clouds and their Nomenclature," by

Cleveland Abbe; "Cloud Photography," by Alfred J. Henry; "A New Apparatus for Studying Color Phenomena," by E. R. von Nardroff; "Voice Production, with Photographs of the Vocal Cords in Action," by F. S. Muckey and William Hallock; "Note on the Limits of Range of the Human Voice," by W. Le Conte Stevens; "Voice Analysis, with Photographic Record," by F. S. Muckey and William Hallock; "Observations on the Relations of Certain Properties of Line Spectra to the Physical Conditions under which they are produced," by J. F. Mohler and W. J. Humphreys; "The Reproduction of Colors by Photography," by F. E. Ives; "Color Definitions for the Standard Dictionary," by William Hallock; "The Significance of Color Terms," by J. H. Pillsbury; "On Standard Colors," by J. H. Pillsbury; "The Analysis of Floral Colors," by J. H. Pillsbury; "On the Comparison in Brightness of Differently Colored Lights, and the 'Flicker' Photometer," by Frank P. Whitman; "Electrolytic Reproduction of Resonators," by William Hallock; "A Photographic Method of Comparing the Pitch of Tuning Forks," by William Hallock; "Illustration of Gems, Seals," etc., by William Hallock; "An Experimental Investigation of the Rotary Field," by H. S. Carhart; "Phenomena with Electric Waves analogous to those of Light with a Diffraction Grating," by C. D. Child; "The Effect of Age upon the Molecular Structure of Bronze, Glass, and Steel," by William A. Rogers; "A New Determination of the Relative Lengths of the Yard and Meter," by William A. Rogers; "An Examination of the Statement of Maxwell that all Heat is of the Same Kind," by William A. Rogers.

A noticeable feature of the meetings of Section B was the extensive use made of the lantern, not only for ordinary projection, but also for purposes of demonstration, as in the case of several of the papers dealing with color. The conditions under which papers are presented to the association render it, in fact, especially desirable that facilities for illustration and demonstration should be at hand. The plan of printing abstracts in advance, which has been adopted with such excellent results by many societies, does not yet appear feasible in the case of the American Association. Few members know even the titles of the papers presented until the day on which they are read. In order, therefore, that the points brought out may be fully appreciated, and that the discussion of a paper may be of value, the greatest possible clearness in presentation is essential. It is for this reason that the use of the lantern deserves to be encouraged; for there is nothing that contributes so much to clearness and concreteness as a well chosen illustration or experiment. In view of the rapid improvement in the provision made for scientific and technical education throughout the country, we may perhaps hope that in the near future Section B will always be able to find a place of meeting that offers *all* the facilities of a modern physical lecture-

room. In the meantime the committee in whose hands is placed the arrangements for the coming meeting should realize that for the physics section the projecting lantern has become a necessity, and should make provision accordingly.

The next meeting of the association will be held in Buffalo, beginning on Monday, August 24, 1896. The officers of Section B are: *Vice-President*, Professor C. L. Mees, of Terre Haute, Ind.; *Secretary*, Professor F. P. Whitman, of Cleveland, O.

The change in the time of the first general session from Thursday to Monday is an innovation which it is hoped will lead to good results. Heretofore the Saturday and Sunday excursions have fallen at such a time as to divide the meeting practically into two; and in spite of the fact that the excursions are always enjoyable, and often possess a scientific interest, it has been the opinion of many that the interruption of the regular business meetings was undesirable. Under the new arrangement, four consecutive days will be available for the regular meetings of the sections.

The change mentioned above in the time of meeting is only one of the changes that were proposed and discussed in the council meetings. Several plans for strengthening the association were suggested and earnestly discussed; but it appeared best to postpone final action until these plans could be more thoroughly considered in committee. If we may judge by the earnestness and enthusiasm which marked the meetings of the council, an increased interest in the work of the association, and in consequence an increase in its usefulness, are to be confidently expected.

MINOR CONTRIBUTIONS.

THE LIMITS OF PITCH FOR THE HUMAN VOICE.

BY W. LE CONTE STEVENS.

IN connection with the interesting experiments on voice production and voice analysis, as communicated by Dr. Muckey and Dr. Hallock at the recent Springfield meeting of the American Association for the Advancement of Science, it may be advisable to record some observations on the limits of range actually reached.

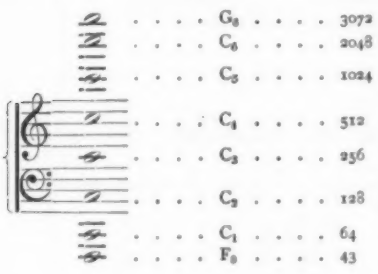
The determination of pitch in vocalization depends usually upon the musical training of the observer. With moderate practice it is quite possible to estimate the vibration frequency with such accuracy that the error is much less than a semitone. Assume that the observer has a tuning-fork of known pitch, such as C_4 (512 double vibrations per second). If the frequency of the tone to be investigated is between 256 and 1024, differing thus less than an octave from the standard, and if it makes a recognizable interval, such as a major third or minor sixth, with the standard taken as keynote, the application of the corresponding numerical ratio gives the pitch at once. If the interval be not exact, its departure from exactness has to be estimated; and here the training of the physicist has to be conjoined with that of the musician. If the observed pitch differ from the standard by an interval much wider than an octave, a tone in unison with it can be instantly sounded with the voice or by whistling. The succession of octaves above or below are then sounded until a pitch is attained that is easily comparable with the standard; or this process may be reversed, the standard being taken as starting-point, and a succession of octaves above or below it quickly furnishes the means for comparison. During my college days I acquired the habit of carrying a tuning-fork (512 vs.); and this habit has been retained to the present day, though the fork is utilized much less frequently than during the earlier period of musical activity. The estimation of pitch by its aid was so much practised as to become very easy, and the extremes of pitch noticed in concert or at the opera were often estimated. When the printed music is at hand, the correctness of such an estimate is readily verified.

The lowest vocal pitch hitherto recorded is F_0, about 43 vs., which is credited to a German basso, Fischer, who lived during the sixteenth century.

In modern opera the basso is rarely ever required to sound lower than C^1 (64 vs.). This limit may be passed by an ordinary masculine voice under abnormal conditions. With the vocal cords thickened by an attack of influenza, I have sounded as low as A_0 (53 vs.), but the sound was faint and of the poorest musical quality.

An ordinary soprano voice may reach C_5 (1024 vs.); and it may be safely stated, in round numbers, that the limits of range of the average adult human voice are from 100 for basso to 1000 for soprano. Adelina Patti is said to sing as high as G_5 (1536 vs.) without sacrifice of good quality. At Parma in 1770 Mozart tested the voice of Lucrezia Ajugari, who trilled on D_5 (1152 vs.), and sang several passages of higher pitch, one of which included C_6 (2048 vs.). An American soprano, Miss Ellen B. Yaw, has lately surpassed this limit, attaining a pitch which is stated to be E_6 (2560 vs.). This statement is made in the advertisement of a concert manager. For the exceptional adult human voice the limits may, therefore, be given as about 50 for basso and 2500 for soprano.

In early childhood all the tissues of the human body are softer and more elastic than after maturity is reached, and it is well known that the conversational pitch in childhood is higher than it is afterward in youth. But I have seen thus far no published record of the highest observed pitch of a child's voice. A few years ago my attention was casually attracted to the squeal of a child at play. The tuning-fork standard at once enabled me to estimate the pitch as G_6 (3072 vs.). This result was so remarkable that, in the absence of any immediate opportunity for verification, I rejected it as possibly the outcome of faulty observation. A few months afterward, while lingering near a group of children at play, I estimated the pitch of quite a succession of squeals, with results varying from 2500 to 3000 double vibrations per second. The observation has since been repeated many times, so that there is no reasonable doubt of the correctness of the first estimate. Under the excitement due to either terror or enthusiasm, the voice may, without conscious strain, reach a limit quite unattainable in song.



These results are conveniently expressed in musical notation. The extreme range is seen to be a trifle in excess of six octaves. For a single voice the range after maturity is rarely greater than three octaves. Ajugari sang as low as G_2 (192 vs.), reaching the phenomenal range of four and a half octaves. Two octaves is the range commonly assigned for the average adult voice.

THE NEW PHYSICS LABORATORY AT LILLE.

BY E. L. NICHOLS.

ON June 1 and 2, 1895, the University of Lille,[1] celebrated by a series of magnificent *fêtes* the opening of seven new buildings devoted to learning. The cost of these, which amounted to 3,500,000 francs, was shared equally by the state and by the city of Lille.

One of the new buildings, the exterior of which is shown in Fig. 1, is an institute of physics. It is not large, compared with many modern European laboratories, but it is well arranged; and it shows in its interior fittings a sumptuousness such as nature, until very recently at any rate, has

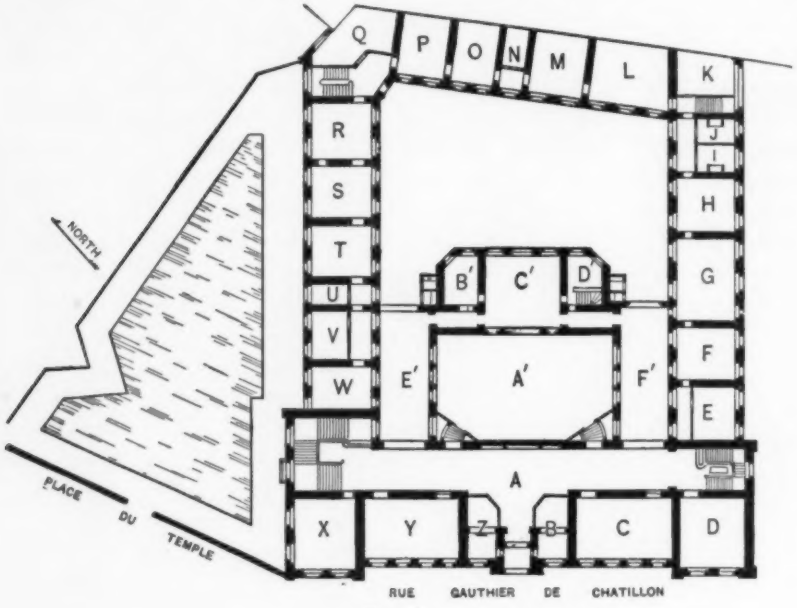


Fig. 3.

been unaccustomed to find in the temples devoted to her service. Some of these decorative features appear in the view of a corner of the large lecture-room, Fig. 2.

The characteristic feature of this laboratory is the subdivision of its interior into many small rooms, each of which is well lighted and easily accessible. These are arranged around a central court.

[1] Officially known as the "Académie de Lille," an institution which, like all the French provincial universities, is affiliated with the University of France.

Fig. 2. — Large Lecture-room.

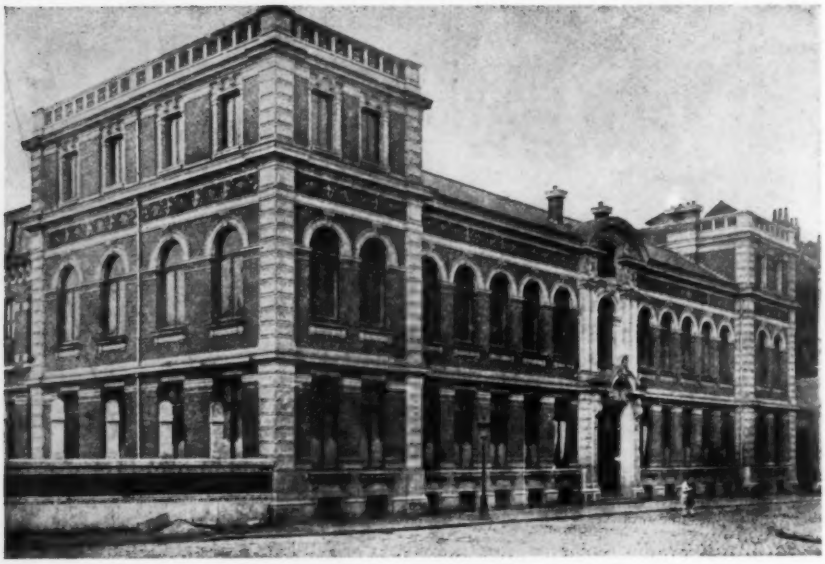


Fig. 1. — The Physical Laboratory at Lille.

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Thus, on the first floor, Fig. 4, there is a suite of nineteen small laboratory rooms, devoted to elementary practice work. In these the apparatus for such work is permanently mounted, there being only one or two experiments in a room. These rooms are not thrown together, but all may be entered from a corridor which serves as means of communication.

On the lower floor, the plan of which is shown in Fig. 3, the corresponding set of rooms is used for research laboratories, offices, etc. There is no corridor, but the adjoining rooms are connected throughout by doors.

This arrangement of many small rooms, within which the multitude of

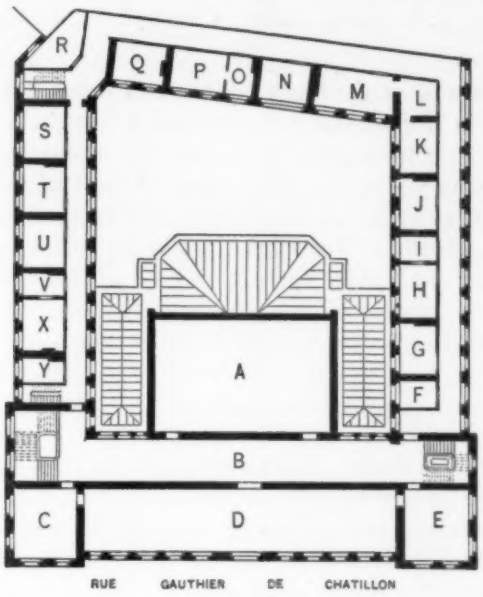

Fig. 4.

special operations of which experimental physics consists may be performed, each in an apartment assigned to it and planned for it, will meet the approval of nearly all physicists.

The Lille laboratory has its own installation of electric lighting. This, like that of nearly all the newer European laboratories, consists of a gas engine, two small dynamos, and a set of accumulators of considerable capacity. The voltage of these batteries is sufficient to serve for arc and glow lighting, as well as for general laboratory purposes.

The low price of gas, and the high stage of development to which both the gas engine and the storage battery have been brought in Europe, render this an economical and a very convenient arrangement.

NEW BOOKS.

Electric Waves, being Researches on the Propagation of Electric Action with Finite Velocity through Space. By DR. HEINRICH HERTZ, Professor of Physics in the University of Bonn. Authorized English Translation by D. E. JONES. 8vo, pp. xv, 278. London, Macmillan & Co., 1894.

At this time of writing, there is small need to describe Hertz's experiments. The term Electric Waves has become a household word, and the demonstration of electromagnetic radiations has become a matter for the popular lecture as well as the class-room. A host of investigators has followed in the steps of Hertz, and this region of research is at present in the heyday of popularity. The ether is a favorite subject of conversation and of publication by authors whose knowledge of mathematics is of the most rudimentary character. This is one of the unfortunate results of the existence of fashions in science, and of the attempt at popularization of difficult subjects by such men as Kelvin and Lodge. Not that the reviewer wishes to discourage popularization, but that it has its disadvantages. One has only to pick up a newspaper or magazine article with the heading "Tesla" to see an example. The present work does not deal with "waggling the charge of the earth," although it contains the account of the original experiments on oscillations of the charges of conductors disturbed from equilibrium, and *measurements* of the action of these oscillations. It is a matter for congratulation that the different researches of Hertz on this subject were collected by him into a volume, and it is a matter for satisfaction to the non-German-reading scientific world (alas that there is one!) to have them translated into English. The title of the work has been changed in the translation, which is also furnished with a preface by Lord Kelvin on the history of the controversy between the adherents of Action at a Distance and the contrary, from the times of Newton and Descartes to that of Hertz. Hertz is indeed fortunate in his preface-writers. To have one's principal works prefaced by the greatest physicists of two of the greatest nations in science does not fall to the lot of many authors, and is not to be lightly valued. In the preface it is stated that Faraday himself, when last seen by Lord Kelvin, was attempting to discover whether magnetic force was propagated with a finite velocity. Nothing came of these experiments, however.

The author is also fortunate in his translator. Mr. Jones has given us an excellent rendition of the sense of the original without considering it necessary to preserve the German spirit by the use of such misshapen English as we often get from enthusiastic pupils of distinguished German professors. Whether it was this fact, or the insight displayed in wishing to present Hertz's work in an English dress, or the experimental work done by the translator under the direction of Hertz, that the Royal Society considered worthy to be honored by a medal, we can only guess.

In the introduction Hertz gives an account of the manner in which he came to make these researches. An account of his first introduction to the subject has been given in a previous number of this journal.[1] The prize question set by the Berlin academy for a relation between electromagnetic force and dielectric polarization of insulators, set him to thinking of what results could be obtained by the use of Leyden jars or the open circuits of induction coils. Consideration seemed to show that the effects desired would be too small to measure. Not discouraged, however, but continually ruminating on the properties of electrical oscillations, a chance observation of a side spark from one of a pair of coils, when a Leyden jar was discharged through the other, set him at work on methodical observations of the phenomenon which contained the germ of his whole subsequent discoveries. His subsequent course of procedure is described with charming frankness, his mistaken conclusions being related with the same readiness as his successful ones, and full acknowledgment being made of the work of other savants with whom he at times found himself in controversy. It is interesting to see how very nearly Professor Lodge came to making the same discoveries as Hertz, who declares that Lodge was on the way to the observation of waves in air, and would probably thus have discovered the finite velocity. The last part of the introduction is devoted to a comparison of the old theories with Maxwell's theory, and to answering the question, "What is Maxwell's theory?" After admitting his inability to understand just what Maxwell intended as the physical representation of his mathematical conceptions, — a state of mind that will be sympathized with by every careful student of Maxwell, — he gives the very sensible answer: "Maxwell's theory is Maxwell's System of equations. Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case of Maxwell's theory." This will be seen to be in line with the views of Helmholtz on mechanics mentioned above. This is a point that cannot be too strongly impressed on amateur theorizers, that the test of a theory is that it leads to the equations which are known to express the facts. If it does not, it is good for the waste-basket. The writer, and, as he supposes, all teachers

[1] PHYSICAL REVIEW, Vol. III. p. 73.

of physics, is in frequent receipt of visits from people with favorite elaborate theories, destined to overthrow Newton, Maxwell, or somebody else, the inventor having invariably no knowledge whatever of mathematics. If these people could be put at some mild employment that would keep them from inventing theories it would be a great gain to the world.

The whole of these fourteen papers, with the exception of the second, on the influence of ultra-violet light in reducing the potential at which discharge takes place, possess a remarkable degree of continuity. Few are more admirable than the first, in which is shown the means of disturbing the charge in a wire conductor so as to cause oscillations, and in which the function of the spark of the induction coil in giving the necessary impulse and lowering the resistance of the air gap enough for the oscillations to occur is clearly described. The existence of wave propagation in the wires is recognized, and the position of the nodes of vibration determined. The existence of resonance was first predicted from theoretical considerations, and then observed and found to increase and decrease precisely as theory demanded.

After admiring the close thinking shown in the performance of these experiments, it comes somewhat as a shock to the reader to find in the next paper quotations from a paper by von Bezold, published seventeen years before, in which many of the same phenomena were observed, the methods of exciting the vibrations being identical. Of the existence of this paper Hertz was at the time of his work of course unaware. It is curious to look over the work of Lodge done at about this time, and to see how similar were the methods used, also with a perfect comprehension of the issues involved. The one thing lacking in Lodge's work was quantitative results of the same order of precision as Hertz's. The present writer remembers that soon after the time of these researches, while he was engaged in demonstrating some of Lodge's experimental results in the laboratory in Berlin, on the occasion of the visit of a number of distinguished physicists connected with the newly established Reichsanstalt, Professor von Bezold seemed quite interested in the experiments, and quietly remarked that he had noticed something similar several years before, giving the reference to his paper. The question naturally arises, how could von Bezold's paper have attracted so little attention, while Hertz produced such a stir years after. Probably because Hertz followed up his researches with others in a logically conducted series resulting in a new discovery. In the next of the series, the effect of the oscillator at various points in space is calculated, and the effects of the magnetic force separated from the electric force by experiments devised with extreme penetration. Next the effect of the electromagnetic action upon insulators is demonstrated, and thus one of the original objects of the investigation fulfilled. Next

by the interference of waves in air with waves propagated in a wire, the existence of a finite velocity is proved, and an attempt made to estimate it by the determination of the wave-length in the wire, the period of the vibration being calculated by the old theory. It is to be noticed that this is the case in the whole series of researches, the calculation of the period depends upon a theory, and hence no direct verification of the velocity demanded by Maxwell's theory would thus be possible. It is worthy of note that neither Hertz nor any of his immediate successors, though they measured wave-lengths without number, ever made a direct determination of the velocity by observing the other needed element, the time. The present writer described such a direct method to Professor Kundt five years ago, and was advised to carry it out, but the facilities not being available, it was put by till a more convenient season. Before this occasion arrived, the experiment had been carried out by Blondlot in the same manner, described in his paper on the determination of the Velocity of Propagation by a method independent of any theory. This was the first and only direct determination until the recent publication of the work of Professor Trowbridge and Mr. Duane, which again anticipated the completion of a precisely similar piece of work done under the direction of the writer. It is, however, desirable to have this velocity determined as often as possible, until it can take its place with other well-authenticated physical constants.

It is in this last research that Hertz had his only disappointment. He thought he had shown that the velocity was different in air and in wires, a mistake that was soon corrected by others. Hertz says that although he may have been lucky elsewhere, certainly here he was unlucky. This is certainly too much modesty. Never could a man with less justice have had his success attributed to luck. With Hertz everything was deliberately planned. After proving the velocity of propagation to be finite, knowing that the waves should be capable of reflection and refraction, he makes them reflect and refract. Knowing that oscillatory currents should not penetrate into the interior of conductors, he demonstrates that they do not. Having calculated what mechanical forces they should exercise, he straightway observes them. These researches are a notable example of how experimental work should be prosecuted, and convey a lesson that should be taken to heart by the student. The proper order of procedure may be stated: "Think, calculate, plan, experiment, think, — and first, last, and all the time, *think*." The method often pursued is: "Wonder, guess, putter, guess again, theorize, and above all avoid calculation." Throughout Hertz's work we are constantly reminded of Faraday by the remarkable acuteness of interpretation of results as well as in the skilful devising of the experiments. Hertz adds to Faraday's experimental skill a

mathematical ability that Faraday did not possess, although he had an intuition which nearly replaced it.

Finally, and, in the opinion of the writer, of most importance, Hertz concludes with two mathematical papers on the fundamental equations, that is, with an exposition of Maxwell's theory. In these he brings out with the greatest vividness the complete duality of all electrical and magnetic phenomena, with the exception of conduction. It is no discredit to Hertz that Heaviside had previously done the same thing with equal clear-sightedness. Heaviside suffers under the disadvantage of having written so much and so diffusely that ten students will read Hertz's papers for one that reads Heaviside's. Maxwell's great work is full of obscurities, incongruities, and contradictions. These he would doubtless have removed in rewriting the work if he had lived. None of his commentators have done it for him, with the exception of these two. Both have adopted unconsciously the same method, namely, that of considering the two vectors, electric and magnetic force, as the essential quantities that are propagated, avoiding introduction of the potentials and vector-potentials, and eliminating altogether the magnetizations. The notation of Hertz commends itself by its perfect symmetry and simplicity. A pair of equations saying that the time derivative of the *induction*, electric or magnetic, is proportional to the curl of the other *force*, another pair saying that either induction is a linear vector function of its own force, — that is all. The real electricity or magnetism is defined by the corresponding induction, the free, or apparent, by the force in the same manner. The treatment of the equations for moving bodies is the most logical that has been given, and by proceeding in a straightforward manner terms are introduced which are lacking in Maxwell's equations. The effect of static charges carried by moving conductors, demonstrated by Rowland's famous experiment, is one of these. There are other effects of a dual nature that are still left to verify, of which more may be said later.

The only fault that can be found with Hertz's notation is that it involves the use of German letters for the components of the inductions. These are inconvenient for writing by Anglo-Saxons, and the writer ventures to suggest a substitution, which he has been accustomed to make use of in his lectures, of the French round letters frequently used by architects in lettering, which are easy to write, not impossible for printers, and easy to name in reading, distinguishing "round $\mathfrak{X}$" and "square X." He hopes that this mild suggestion will not be run afoul of by some member of the American Institute of Electrical Engineers, stating that M. Hospitalier has already proposed these for something entirely different.

In conclusion, let us advise the student to place this volume on his shelves with its first, experimental side next to Faraday's Experimental

Researches, and with its second, or mathematical part, next to Maxwell's Treatise, "for on these three hang all the law and the prophets" of electricity.

ARTHUR G. WEBSTER.

Mechanics, an Elementary Text-Book, Theoretical and Practical, for Colleges and Schools: Dynamics. By R. T. GLAZEBROOK, M.A., F.R.S. 8vo, pp. xii+256. Cambridge, the University Press, 1895.

The past year has been prolific of works on Physical Science. Having come to realize that existing text-books are of doubtful utility, because of their ancient character, the scientific world has aroused itself to the task of producing new ones that are at once the result and the harbinger of a more practical method. The text-books by R. T. Glazebrook are especially deserving of notice. The one mentioned above is one of a series by this writer which bear the same general character as to method, and which give promise of covering quite thoroughly and efficiently that portion of physics usually taught as a Sophomore or Junior course in college.

The conviction is at once forced upon the reader that the book in question is the outgrowth of the author's method of teaching, and he is therefore the more gratified on turning to the preface to read: "The portions of the following book designated Experiments have, for the most part, been in use for some time as a Practical Course for Medical Students at the Cavendish Laboratory." . . . "The rest of the book contains the explanation of the theory of those experiments, and an account of the deductions therefrom." In fact, it would seem that the author's method of presenting physics to a class is the best calculated to secure results, and is at the same time within the reach of any teacher, and of any institution — even one of limited resources. The author develops the theory, and immediately proceeds to give definite instructions for its verification and practical application, describing in connection therewith apparatus suitable for the purpose, of so simple a character that it can be easily set up and adjusted in any laboratory. The same apparatus, moreover, is suitable for lecture-room purposes; for in the preface we find the statement: "most of the lecture-room experiments are performed with the apparatus which is afterwards used by the class, and wherever it can be done, the theoretical consequences are deduced from the results of these experiments." This plan seems an ideal one for teaching physics, giving, as it does, to the lecture, recitation, and laboratory a unity of character that can scarcely be obtained otherwise; and it is no small advantage in a book that makes this method possible. The style of the book is best described as tangible.

A first book in Mechanics should appeal to the experience of the student, for the science itself is built upon the everyday experiences of life. In expounding Newton's Third Law, he makes clear what is meant by "Action" and "Reaction," nor does he consider it undignified to appeal to the case of horse drawing the canal boat, and even the tyro in mechanical science cannot fail to see clearly the application.

The classification of the subject is a happy one. The first chapter is devoted to measurements of length, area, and volume. Two chapters are then taken up by Kinematics — Velocity and Acceleration, respectively. Momentum is then presented in an inductive manner, while the fourth chapter deals with rate of change of momentum, — Force. The sixth, seventh, and eighth chapters are devoted to Newton's Laws of Motion, the last one including, naturally enough, the treatment of energy. In the ninth chapter curvilinear motion is presented, which subject includes quite a full treatment of Projectiles. The tenth chapter is devoted to Collisions, while the eleventh, entitled "Motion in a Circle," deals with a variety of subjects, including the Hodograph, Circular Motion, Simple Harmonic Motion, and The Pendulum.

The chapters on force and energy are especially clear and full. The treatment of Newton's Laws is no less good, as is also that on Collisions. It would seem, however, from the small amount of space devoted to the subject, that the author underrates the importance of simple harmonic motion. The same criticism might well be passed upon his treatment of the pendulum; and an even more glaring defect is the entire absence of any mention of the moment of inertia and the whole subject of moments. In fact, it would seem that after a most excellent treatment of the subjects included in the first ten chapters, the publication of the book was hurried to the extent of grouping all that was left of mechanics — not a small portion — in the last, the eleventh, chapter.

The book is remarkable for its accuracy, both of principle and detail. Seldom does it happen that a first edition does not present errors to be corrected in a second. The absence of even typographic errors makes the book conspicuous in this particular. The excellent historical treatment of the subject is also to be noted. Nor should a review of the book omit mention of the excellent collection of problems included at the close of each chapter.

Text-books on this subject are generally too simple or too elaborate for a conception of elementary mechanical principles. This book cannot fail to recommend itself, therefore, for a first course, preliminary to the study of physical science. No other book presents in the same space, with the same clearness and exactness, so large a range of mechanical principles.

HENRY E. LAWRENCE.